PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIV.

PART I.

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MDCCCXXXIV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

27707

upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

I. *On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath. By* CHARLES DAUBENY, *M.D. F.R.S. Professor of Chemistry in the University of Oxford* . . . . page 1

II. *On the Empirical Laws of the Tides in the Port of London; with some Reflexions on the Theory. By the Rev.* WILLIAM WHEWELL, *A.M. F.R.S. Fellow and Tutor of Trinity College, Cambridge* 15

III. *On the Position of the North Magnetic Pole. By Commander* JAMES CLARK ROSS, *R.N. F.R.S. F.R.A.S. F.L.S. &c.* 47

IV. *Notice as to the supposed Identity of the large Mass of Meteoric Iron now in the British Museum, with the celebrated Otumpa Iron described by* RUBIN DE CELIS *in the Philosophical Transactions for* 1786. *Communicated in a Letter from* WOODBINE PARISH, *Esq. F.R.S. to* CHARLES KÖNIG, *Esq. Foreign Secretary of the Royal Society* 53

V. *Experimental Researches in Electricity.—Sixth Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.* 55

VI. *Experimental Researches in Electricity.—Seventh Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.* 77

VII. *On the Theory of the Moon. By* JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.* 123

VIII. *On the Theory of the Moon. By* JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.* 127

IX. *On the Tides.* By JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.* . 143

X. *On the Nature of Death.* By A. P. W. PHILIP, *M.D. F.R.S. L. & E. &c.* . 167

XI. *An Account of a Concave Achromatic Glass Lens, as adapted to the Wired Micrometer when applied to a Telescope, which has the property of increasing the magnifying power of the Telescope without increasing the diameter of the Micrometer Wires.* By GEORGE DOLLOND, *F.R.S. &c.* 199

XII. *On the Principle of Construction and general Application of the Negative Achromatic Lens to Telescopes and Eyepieces of every description.* By PETER BARLOW, *Esq. F.R.S. &c.* 205

XIII. *Some Suggestions relative to the best Method of employing the New Zenith Telescope lately erected at the Royal Observatory.* By JOHN POND, *Esq. A.R. F.R.S.* 209

APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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The Geological Society.
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The Royal Academy of Sciences at Thoulouse.
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The Royal Academy of Sciences at Berlin.

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The Royal Academy of Sciences at Stockholm.
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The University of Glasgow.
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The Observatory at the Cape of Good Hope.
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The American Academy of Sciences at Boston.
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The Observatory at Berlin.
The Observatory at Brussels.
The Observatory at Cadiz.
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The Observatory at Dorpat.
The Observatory at Königsberg.
The Observatory at Manheim.
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The Observatory at Milan.
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PHILOSOPHICAL TRANSACTIONS.

I. *On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath. By* CHARLES DAUBENY, *M.D., F.R.S., Professor of Chemistry in the University of Oxford.*

Received November 2,—Read December 19, 1833.

IN some remarks on a paper of Dr. JOHN DAVY's, entitled "Notice on the Remains of the recent Volcano in the Mediterranean," which were inserted in the last Part of our Transactions, I expressed my regret, that no accurate statement of the quantity of gas evolved in a given time from any thermal waters had been published, and intimated my intention to make this the subject of special examination, whenever suitable opportunities should occur.

Accordingly, having in my former visits to Bath been struck with the copious evolution of gas from the centre of the principal of the hot springs of that city, I solicited from the gentlemen composing the Committee appointed to manage and regulate them, leave to institute such experiments as appeared to me desirable on the spot; and having obtained from them the requisite facilities for so doing, I determined to collect and measure the gas evolved, repeatedly, during a period sufficiently extended to enable me to fix with tolerable precision its average amount, and to ascertain, whether any greater diurnal variation in its quantity could be detected, than what might be fairly set down to errors of manipulation, or to oscillations in the quantities discharged extending over a wider range of time, than that during which it might be found convenient to protract the period of each observation.

Anticipating, also, that a variation might be discovered, I carried on during the same period a corresponding register of the leading conditions of the atmosphere as to pressure, temperature and humidity, in order to learn, whether any connexion could be traced between these and the quantities of gas evolved.

I likewise examined, on several occasions, the quality of the gas emitted, not only in order to fix with greater exactness its actual composition, but likewise to learn, whether any variation in these respects could be perceived during the course of my observations.

The Bath waters arise from three distinct sources, or springs, contiguous to each

other, which supply respectively the King's Bath, the Hot Bath, and the Cross Bath. In addition to the above, which belong to the Corporation, there is likewise said to be another spring of water, which supplies the Kingston Baths, the property of an individual.

Of these by far the most copious is the first-named, which fills the King's Bath and the Queen's Bath annexed; it gives out in a regular and unremitting stream no less than 126 gallons of water per minute, and in nine hours replenishes the whole area of these two baths, when emptied, to the height of forty-six inches.

The evolution of gas takes place chiefly from this spring, the quantity discharged by the Hot and Cross Baths being comparatively insignificant; for I found that the latter of these, which, of the two, is considered to yield the most, disengaged no more than about twelve cubic inches a minute at the time I attempted to estimate it.

I therefore confined my inquiry to the King's Bath, from the centre of which gas rises in great quantities, whilst it is also given off in a slighter degree and in a more irregular manner, from the various holes and crevices, that exist in the stone pavement of the bath throughout the whole extent of the area embraced by it.

To form an exact estimate of the amount of the gas which escapes from these minor lateral spiracles, would have been an irksome and difficult task; but I ascertained, that it bore but a very small proportion to that discharged from the centre, and from a certain distance round it, being in the one case emitted in bursts at uncertain but distant intervals, in the other proceeding in a current as regular and unintermitting as the spring itself.

I therefore attempted to do no more, than collect that portion of the gas which finds its way upwards, from an area round the centre of the bath, about twenty feet in diameter*: and in order to accomplish this, I contrived a funnel-shaped apparatus, which for brevity's sake I shall in the rest of this paper call the Shield, consisting of several sheets of iron riveted together, and rendered airtight by means of white lead interposed between the seams of the joints, so that there might be no means of escape for the gas detained under the lower surface of the shield, except at its centre, where an aperture of two inches in diameter was left, towards which it would be conducted, and thus find a ready vent. The apertures in the pavement of the bath within the area above specified, which the shield was not large enough to cover, were carefully stopped up either with corks or tow, covered over with boards, which by means of weights were made to press closely upon the surface of the pavement.

The apparatus being thus arranged, it was easy, by placing inverted jars of suitable dimensions filled with water over the central orifice, to collect and measure the gas that might escape within a given period.

The iron shield being six feet square, and therefore from its size somewhat unwieldy, it was found convenient to erect a kind of temporary framework of wood, from which the apparatus, when not in use, might be suspended out of the way of bathers.

* See the ground plan of the bath.

This consisted of four upright posts, fixed by means of cords to opposite angles of the hand-rail which separates the central portion of the King's Bath from its sides; these posts being connected together by planks extending from one to the other, and resting horizontally on the tops of each.

This framework served as a point of attachment to the pulleys by means of which the iron shield was raised and depressed; and it at the same time enabled me to cover over that portion of the bath in which my operations were conducted, with an awning, to serve both as a protection from the weather, and as a screen from observation.

In this manner I was enabled to carry on my experiments for the period of a month, without being any hindrance to others, and with comparative ease to myself and my assistants, choosing a time of day after the hours of bathing were concluded, when the water was comparatively low, which, as the bath is emptied regularly once a day, and afterwards replenished from the spring at the uniform rate of nearly six inches per hour, could always be reckoned upon at a similar time in the afternoon.

On one occasion, however, I caused the water to be kept low during the space of twenty-four hours, in order to satisfy myself that there was no material deviation from the mean quantity of gas evolved each day, at the particular period that had been commonly selected; and for this purpose I collected the gas at intervals of about six hours, namely on Saturday at 1 P.M., again at 6 in the evening, again about midnight, again at 7 o'clock on Sunday morning, and lastly at noon of the same day. Finding, however, a greater variation between the quantity collected at the same hour on the two successive days, than between that obtained at different hours of the same day, I saw no advantage in departing again from the usual routine.

There might be more reason for suspecting, that the periods to which each observation extended were not sufficiently long to secure a fair average; and fearing that this might have been the case in the first trials, I gradually prolonged the duration of them from five to fifteen minutes: so that the time occupied in the experiments amounted altogether to more than four hours.

If it be objected, that this period even might have been too short to insure exact results, I may reply, that the correspondence which is frequently to be traced between the amount obtained on two or even three successive days, affords in itself a presumption, that the numbers given furnish in general a pretty fair expression of the mean quantity of gas emitted every minute from the spring.

Assuming this to be the case, it would appear from the sum total of the observations recorded, that the quantity of gas evolved averages 264 cubic inches, or, rejecting observation 3rd, (which was noted at the time as of doubtful accuracy, on account of some defect in the apparatus subsequently remedied,) 267 cubic inches each minute.

The largest quantity ever obtained in the course of one minute appears to have been 530 cubic inches; the smallest in the same time, 80: but if we take the mean of the observations, we shall find, that the quantity usually varied from 339 to 207, so

that on an average the volume of gas evolved differed between one minute and others as 12 to 20, or about one third, whilst the extreme variation was as 3 to 20, or four times as great.

By comparing together periods of five minutes, the difference between one observation and another will be found greatly reduced, the greatest variation being no more than as 14 to 20, the mean only 19 to 20, as I have calculated by referring to my original notes, and taking at hazard eleven observations, each extending to ten minutes, and comparing the amount of gas obtained in the former half with that in the latter half of the time.

I conceive, therefore, we may be warranted in calculating, on the above data, that no less than 223 cubic feet of gas are usually disengaged in the space of twenty-four hours from this source alone; and, large as this amount may appear, we have positive testimony* that it was as constantly taking place almost a century and a half ago; nor is there much reason to doubt that it may have continued in an equally unintermitting flow from the earliest periods at which the springs were known: the analogy, indeed, of other thermal waters leading us to conclude, that the evolution of gas is a phenomenon as intimately connected with the constitution of the waters as the presence of a definite quantity of certain saline ingredients, or the possession of a particular temperature; both which, it is probable, continue unaltered during periods, historically speaking, of long duration.

Dr. Clarke has shown†, that the hot springs which gush out from the foot of the limestone precipices of Mount Œta at the pass of Thermopylæ, retain at this moment a temperature of 111°, which probably is as high as that belonging to them in the times of ancient Greece; and, what is more to the point, the boiling up of gaseous bubbles, which this traveller ascertained to be owing to the escape of sulphuretted hydrogen from the springs, would seem to be noticed by Sophocles, who, if we adopt Dr. Clarke's ingenious interpretation of the passage referred to‡, makes a poetical use of the phenomenon in his Trachiniæ.

Thus, too, Dr. Holland detected, at a spot on the coast of Albania, the escape of carburetted hydrogen, probably on the precise spot where the ancient writers describe a Nympheum, or a place where, owing to this cause, a perpetual flame was observed to rise§: and the baths of Bithynia, described by a poet of the time of

* Guidot, who wrote on the Bath waters in 1696, speaking of the ochreous sediment thrown up by the spring, says: "ochra hic cum scaturigine regiâ erumpens jugiter, *perenni ebullitionis motu*, ita elaboratur, ut tabulis affixa pictoriam æmuletur."

† Travels, vol. iv. p. 248.

‡ He causes Deianira to relate, that some of the wool stained with the blood of the Centaur Nessus, falling upon the Trachinian plain, in a place where the sun's rays were the most fierce, there boiled up from the earth *frothy bubbles*.

..... ἐκ δὲ γῆς, ὅθεν
πρόσκειτ', ἀναζέουσι θρομβώδεις ἀφροί.—Sophoclis Trachiniæ, vers. 701. (Ed. Brunck.)

§ See Holland's Travels in Greece and Albania.

JUSTINIAN* as discharging a hot bubbling fluid†, which (as may be gathered from another passage of the same poem‡,) appeared to boil up, owing to the escape of bubbles of sulphuretted hydrogen, are still found to be impregnated with this same gaseous ingredient §.

To return to the case of the Bath waters, there is no evidence, that it has at any time been the practice to cool them down artificially, before they were employed for bathing; and hence it seems fair to conclude, that they never, since the earliest period at which they have been known, were hotter than they are now, their present temperature probably approaching the highest point which the human body can sustain without inconvenience. From the permanency of the temperature, therefore, we may be led to presume that of the other properties which at present characterize the spring.

It may also be alleged, that geology supplies us with examples of hot springs, of many distinct epochs; some, like that of Castellamare near Naples, connected with the volcano there existing; others, like that of Mount Dor in Auvergne, with volcanos long extinct, but which yet are not more remote than the tertiary period at farthest; whilst a third class, like those of Bath and Buxton, probably are of a much greater antiquity. Yet in all these cases the same evolution of nitrogen is observed; so that we are not at liberty to consider this, as a phenomenon resulting from any one particular period in the existence of a mineral spring, but as one continuing in it from first to last, or at least during a space of time of very extended duration.

Perhaps we shall best explain this regular and long-continued evolution of elastic fluid from the bowels of the earth, by supposing a large amount of these gases to be pent up in some cavern existing in a rock, which is seated at a great depth below the surface, and which had been heated at some former period by volcanic action.

If such a mass of rock were of considerable dimensions, and consisted of materials which slowly transmitted heat, we might suppose its external portions to cool far more rapidly than the internal ones; in which case, the former contracting upon the

* PAULUS SILENTIARIUS (viz. Silence-keeper in the imperial palace at Constantinople). See BRUNCK's Analecta, vol. iii. p. 94, a poem entitled εις τα εν Πυθιοις θερμα.

† ουτω προηλθε πᾶσι
το θερμοβλυστον ῥεῖθρον,
Ἱπποκρατης ἀψυχος,
τεχνης ἀνευ Γαληνος.

"'T was thus the hot bubbling fluid issued for the benefit of mankind, an inanimate HIPPOCRATES, a GALEN untaught by art."

‡ Thus he supports his theory as to the cause of this and other thermal waters, by alleging the mephitic offensive stench which accompanies them:

ὀδμη γαρ εστιν, οἶδας,
μυδῶσα, δυσπνοουσα,
τρανον τε μαρτυροῦσα.

§ See, in WALPOLE's Memoirs on Greece and Turkey, a notice of these springs by the traveller BROWNE.

latter, and thus creating a pressure upon the contents of its included cavity, would prove the means of propelling a stream of air, proportionate to the degree of diminution that had taken place in its own temperature, through fissures towards the surface.

In some such way, perhaps, we may imagine the evolution of gaseous matter to go on for centuries, in a manner nearly as uniform, as the shrinking in the dimensions of the cavity, caused by the yielding of its walls to the pressure from without, may be supposed to proceed*.

Be that as it may, the above estimate of the quantity discharged will afford a standard, by which a comparison may be made at present between this and other thermal springs in the above respect, and which may be appealed to hereafter, should it be wished to ascertain, from time to time, whether any change has taken place in the nature of this particular spring, or in the causes from which its heat proceeds.

Whilst engaged in thus determining the aggregate amount of all the gas evolved by the spring, saving the small quantity that finds its way from apertures near the sides of the bath, by means of the apparatus above described, which enabled me to collect whatever was disengaged within an area of twenty feet, I conceived that it might be worth while at the same time to estimate, what proportion of the whole rises up immediately through the stone cylinder, eighteen inches in diameter, which exists in the centre of the bath.

This was readily ascertained by means of a smaller shield, or funnel-shaped apparatus, which exactly fitted that opening, and the results obtained in the space of each minute are accordingly registered in separate columns by the side of the former.

The mean quantity obtained, taking the average of nineteen observations, was 34·75 cubic inches; the maximum ever obtained, 80; the minimum, 5; the average variation

* The dimensions of such a cavern, or series of caverns, need not be supposed so much more considerable than those of many which have fallen under our observation, as to give rise to any serious difficulty. That at Speedwell Mine, in Derbyshire, contains a pool of water 320 feet in depth, and rises to a height of more than 450 feet above the surface of the water. It is therefore nearly 800 feet in perpendicular height. That at Adelsburg, in Carinthia, one only of a series existing in that limestone formation, is in many places more than 100 feet in height, and extends, it is said, to a distance of nine or ten miles. One lately noticed by a traveller in the Caucasus (Colonel Monteith) is 600 feet high, 1200 feet in span, and 800 feet in depth. It probably communicates with others by means of fissures. See Geographical Journal, vol. iii., lately published. Now, if we suppose 250 cubic feet of gas to have been expelled daily from a cavern underneath Bath, for a period of about 5000 years, the whole quantity given off would amount to 456,250,000 cubic feet. A cavity, therefore, equal to 2000 cubic feet in its entire dimensions, if in the course of that period it had contracted to $\frac{1}{1}\frac{1}{1}$th of its original size in consequence of the cooling of its walls, would have expelled a quantity of gas corresponding with that which the Bath waters have disengaged within that period. But a contraction of $\frac{1}{1}\frac{1}{1}$th may without difficulty be imagined to have resulted from the cooling down of a mass of rock from 13,000° Fahr. to 400°; for glass contracts about $\frac{1}{1}\frac{1}{1}\frac{1}{1}$dth part between 212° and 32°, or by cooling 180°; consequently a rock which contracted in an equal ratio with glass would diminish in bulk $\frac{1}{1}\frac{1}{1}$th by an abstraction of heat equal to 12,600°, assuming even that the expansion at elevated temperatures is not more considerable than it is at low ones.

between the quantity at one minute and another appearing to be as 53·5 to 21·5 cubic inches, or as 5 to 2. So that the quantity evolved from the central orifice seems to be about one seventh of that from the area from which the gas was before collected.

Upon reviewing the aggregate of the observations above detailed, I cannot bring myself to believe, that the gas at present disengaged in a given time from the King's Bath is to be regarded as invariable in quantity; for the differences between the results obtained on one day and another are too considerable to be referred to errors of manipulation, or to the escape of the gas in a greater or less degree from other avenues. Besides, there will be seen, by referring to the annexed Table, a kind of flux and reflux in the quantities obtained; that of September 17th exceeding the mean by no less than seventeen cubic inches, those of the 18th, 19th, 20th, 21st, 23rd falling short of it by variable quantities; that of the 24th exceeding it again; on the 25th and 26th approaching it very nearly; on the 27th, and again the next time of observing, namely on October 2nd, exceeding the mean; from thence till the middle of the 5th falling short of it a little, then till the 9th exceeding the mean, on the 17th falling short of it again, but on the 18th again rising somewhat beyond the average.

If, then, a variation in the quantity of gas emitted seems to be fairly substantiated by the observations I have recorded, it becomes a subject for inquiry, to what cause this irregularity may be ascribed.

I at first imagined, that its emission might be in some degree controlled by atmospheric pressure; but the general tenor of the observations seems to dispell this notion, or at least to show, that there are other causes by which its flow is in a greater degree affected. Neither do the other conditions of the atmosphere noticed in the Table appear to exert any appreciable influence upon the current of gas, though, as the weather during the time I spent at Bath was in general fine, and during a large portion of the time remarkably steady for the season, it were to be wished that some gentleman resident on the spot would avail himself of the opportunities that might present themselves for examining the spring under a greater variety of circumstances; especially, as it has been vaguely stated with regard to some other hot springs, on the authority of casual observers, that the evolution of gas is greatest during storms and gales of wind*.

With regard to the quality of the gases given off, I have but little to add to what had been before determined. In the air I collected, oxygen certainly was present, as indeed Sir G. Gibbes had already ascertained to be the case, by the test of nitrous gas. Phosphorus heated in a bent tube with a measured portion of the air causes a diminution in its volume, which in almost all my trials amounted so nearly to 1·25 per cent., that I set down the proportion of those two gases one to the other as probably constant; and if we grant that nitrogen obtains an increase of bulk amounting to 2·5 per cent. by phosphorus vapour, I cannot be far wrong in reckoning

* The same thing has been noticed with regard to volcanos. See Scrope's Considerations on Volcanos, p. 7.

the oxygen at rather less than four parts, and the nitrogen as rather more than 96, in the 100. This, however, is to be understood as only applying to the gas remaining after having been agitated with a solution of potash; for there is always present a certain portion of carbonic acid, which on some occasions appeared to amount nearly to what Mr. R. PHILLIPS in his analysis has stated it at, namely at 4·5 per cent. of the whole quantity.

I have at other times, however, found it to reach 8 or 9, and once even 13 per cent.; so that I am forced to conclude this element in its composition to be of variable amount.

As, however, the quantity that escapes is only the excess over and above that which the water itself holds in solution, the cause of its variation may perhaps be explained, without imagining it an indication of any change in the nature or intensity of the processes to which the heat of the springs is owing.

The quantity of water which is discharged per minute has been calculated at about 146 gallons, of which 126 gallons are received at the King's and Queen's Bath, 10 at the Cross Bath, and about 10 at what is called the Hot Bath.

Now it is stated by Mr. PHILLIPS, that every pint of the water contains 1·2 cubic inch of carbonic acid; so that about 1400 cubic inches of carbonic acid are dissolved in the water, whilst the quantity that escapes may vary from about 12·3 to 36·0 cubic inches*.

It is evident, therefore, that an increase in the supply of water from 146 to 150 gallons would occasion the whole of the gas to be absorbed, and that a difference of only two gallons and a half in the amount of the water discharged would account for the utmost variation in the quantity of carbonic acid, which I have ever detected between one day and the next.

Now, though the supply of water is remarkably uniform, I conceive, that so slight a variation as that hinted at, might easily take place without its being observed†.

It might be worth while to ascertain, whether the quantity of gas evolved bears any relation, either to the temperature or the copiousness of the spring from which it rises; and, so far as the thermal waters of this country are concerned, it would appear,

* Mr. WALCKER's analysis (Journal of Science for June 1829,) differs from the above, as he calculates only 0·95 of a cubic inch of carbonic acid in every pint of the water, or 7·6 cubic inches to the gallon. Even then, however, 957·6 cubic inches of gas would be given off every minute by the spring, even if we do not count any portion of that which exists in combination with the iron, lime, or alkalies, as derived from the interior of the earth. The latter Mr. WALCKER has stated at 1·62 cubic inch in every pint.

† This, however, assumes, what perhaps will not be granted, namely, that the carbonic acid present in the water is derived from the gas emitted from below, and not from the atmosphere. A cold spring from the lower part of the city close to the Cross Bath, yielded me in the pint,

Carbonic acid	0·90
Nitrogen	0·64
Oxygen	0·13
Total	1·67 cubic inch.

that such a conjecture is somewhat contenanced by what is observed at Bath, where the hottest and most abundant emits the largest quantity of elastic products.

Thus the King's Bath, which possesses, as I have ascertained by almost daily observations for a month by a thermometer with a scale divided to half-degrees of FAHRENHEIT, an uniform temperature of 115°, and which evolves 126 gallons of water per minute, disengages on the average about 240 cubic inches of nitrogen, whilst the Cross Bath, which affords only about eight gallons, and is at 96°, gives out only 12 cubic inches of gas. What the quantity may be from the Hot Bath, which, besides being hotter, is also somewhat more copious than the Cross, I have not had the means of correctly ascertaining, as, at the spot where it issues from the earth, it is covered over; but I have reason to believe that the emission of gas from it must be small.

The only other warm spring, which I have as yet examined with reference to this point, is that called Taafe's Well, already noticed as occurring near Cardiff, in which the thermometer rises only to 70, and this, which discharges much less water than the others mentioned, gives out only 22·5 * cubic inches per minute†.

But we ought not to build on so scanty an induction of particulars, and must pause for the present, in the hope, that in other countries those who may be favourably circumstanced for such inquiries will repeat, with reference to the thermal springs of the Continent, the same observations which I have undertaken at Bath.

Having now, as I hope, faithfully recorded the limits, within which the quantities of elastic fluid evolved by the principal Hot Spring at Bath appeared to fluctuate, during the period of my observations, and submitted to the consideration of the members of this Society a mode of accounting for its regular and nearly equable disengagement, I shall forbear to speculate on the causes of its peculiar chemical constitution, or to dwell upon the inferences that might be deduced from its presence, with regard to thermal springs in general.

I will only remark, that the largeness of the volume of nitrogen gas which is disengaged, and the entire absence of carburetted, sulphuretted and phosphuretted hydrogen, seem to afford an additional presumption against the idea, advanced by a distinguished chemist in a paper recently published in our Transactions, that the nitrogen gas which escapes from volcanos and from thermal springs may be derived from the atmospheric air, held in chemical solution by water generally, but deprived in these instances of the greater part of its oxygen by animal and vegetable putrefaction. It seems obvious, that no amount of water, which can be supposed to obtain access to the depths at which the heat originates, could be sufficient to supply so

* This must be considered only a rude approximation, as I had no apparatus large enough to cover over the whole of the bath, and consequently to collect all the gas that rises at once.

† This gas contained no carbonic acid, but consisted of

Oxygen	3·5 (allowing an expansion of 2·5 for phosphorus vapour, as in the other cases,)
Nitrogen	96·5
	In the 100 parts.

large a quantity of nitrogen, and likewise that no such quantity of vegetable and animal matter could there exist, as would be requisite in order to absorb a corresponding proportion of oxygen.

Neither in such a case could the nitrogen that escaped arrive at the surface, without being contaminated with some of the inflammable products, that commonly arise from the decomposition of organic matter.

I look, therefore, to some process of combustion, during which the atmospheric air that finds admittance is in great measure deprived of its oxygen, as a likelier mode of accounting for the peculiar constitution of the gas emitted; and conceiving that the carbonic acid that accompanies it, is more probably derived from the calcination of earthy carbonates, than from the combustion of beds of coal or bitumen, I am led to conclude from the frequent absence of other gaseous products, that the oxygen becomes united to some base, which forms with it a compound not easily volatilized by heat.

How far these conclusions, if considered to be substantiated, tend to support that theory of volcanos and the connected phenomena, which naturally emanated from the discoveries of our former illustrious President, who also, at one period at least of his life, himself advocated it, must be left for the Society to decide, as it would ill become me to do more, than to lay before its members a statement of such facts, as appear to bear upon a question, respecting which the highest authorities in science have been divided.

In conclusion, therefore, it only remains, that I should express my obligations to the gentlemen who constitute the Committee of the Bath Waters, to whom I applied for leave to institute the above observations, for their ready acquiescence in my wishes, and for the facilities afforded me in the prosecution of these researches.

I must, likewise, acknowledge the kind assistance I received from several of the residents of Bath, particularly from Mr. G. Spry, who has long taken an active concern in the conduct and management of these springs; and from Mr. Thomas Stephens Davies, Fellow of the Royal and the Astronomical Societies, a gentleman well known for several valuable mathematical papers, to whom I am indebted, not only for much occasional information relative to the springs, but also for having, at a considerable sacrifice of time and convenience, attended at the bath whenever the observations were made, and taken upon himself the task of minuting their duration, and of noting down with the utmost regularity the quantities of gas each time obtained.

REGISTER of the quantity and quality of the gases evolved by the hot spring which supplies the King's Bath in the city of Bath, at the times and under the circumstances stated below.

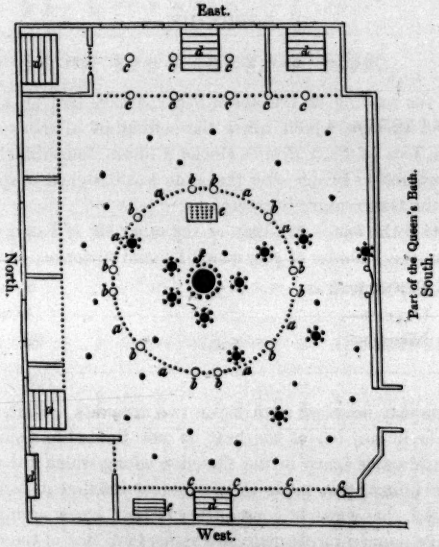
Obs.	Date.	Hour.	Weather.	Barometer.	Thermometer, FAHR.	Dew point.	Gas collected per minute from the central orifice.			Gas collected per minute from an area of twenty feet in the centre of the bath.			Amount of carbonic acid per cent. each day.	Duration of each observation.
							Max.	Min.	Mean.	Max.	Min.	Mean.		
	1833.													Minutes.
1.	Sept. 17.	4 P.M.	Sunshine and rain alternately	29·525	60	.....	.....	.....	28	.....	.....	280	8·00	5
2.	18.	2 P.M.	Sunshine and rain alternately	29·7	64	50	.....	.....	28	.....	.....	253	9·25	5
3.	19.	1 P.M.	Sunshine, followed by storms of rain	29·9	66	52	80	15	39	.....	.....	194	.....	6
4. ☽	20.	1 P.M.	Dull morning, followed by bright sunshine	30·1	60	49	65	20	39	.....	.....	230	.....	7
5.	21.	1 P.M.	Much as the 20th	30·0	66	52	55	20	30	.....	.....	253	13·50	6
6.	23.	1 P.M.	Fine bright sun during most of the day	29·7	66	49	50	30	40	280	80	247	.....	10
7.	24.	1 P.M.	Stormy morning; cloudy afternoon	29·2	62	58	45	35	40	310	220	276	.....	10
8.	25.	1 P.M.	Storms of rain, and sunshine, alternately	29·555	61	48	40	30	35	300	220	266	.....	10
9.	26.	1 P.M.	Bright sunshine, and cloudless sky	29·65	61	48	40	30	35	300	220	266	.....	9
10.	27.	1 P.M.	Bright sunshine as on the 26th	29·72 *	59	49	50	10	30	345	220	276·5	.....	12
○	28.													
11.	Oct. 2.	between 1 & 2 P.M.	Bright sunshine, and cloudless sky	30·05 †	63	47	60	15	35	330	240	274·5	.....	10
12.	3.	between 1 & 2 P.M.	As on the 2nd	30·00	58	51	50	20	30	310	215	266	4·60	10
13.	4.	1 P.M.	As on the 3rd	29·975	59	49	60	10	36·5	330	200	266·5	4·66	10
14.	5.	1 P.M.	Occasionally overcast, with gleams of sunshine alternately	30·0	58	47	50	23	40·5	310	220	262	9·00	10
15.	.....	6 P.M.	Calm, but cloudy evening	30·0	54	.....	.....	.....	.....	350	200	265	.....	11
16.	.....	12 midnight	Calm, but cloudy evening	.....	.....	.....	.....	.....	.....	340	250	280·5	.....	10
17. ☾	6.	between 7 & 8 A.M.	Fine bright morning	.....	.....	.....	.....	.....	.....	530	220	300·5	4·60	13
18.	.....	1 P.M.	Fine sultry day, somewhat less clear than the preceding ones	29·9	59	49	40	15	30	370	200	295	.....	12
19.	7.	1 P.M.	Dull morning; fine afternoon	30·0	57	47	65	5	30	325	235	272	4·50	15
20.	8.	1 P.M.	Dull morning; fine afternoon	29·95	55	43	40	30	33	320	180	271·5	9·00	15
21.	9.	between 1 & 2 P.M.	Very bright morning; duller afternoon, with fleecy clouds, but sunshine.—Wind changed to the west	30·05	58	45	80	35	48	300	215	256·6	9·00	15
●	13.													
22.	17.	1 P.M.	Bright morning; afternoon overcast.—Wind north	29·625	56	.....	.....	.....	.....	355	180	244	.....	10
23.	18.	Both obs. betwixt 1 and 2 P.M. at intervals of 10 min.	Dull and damp morning	29·5	54	47	40	25	33·3	400	200	266	.....	10
24.	.....		Rain in the afternoon	.....	.....	.....	.....	.....	.....	350	230	270	5·66	10
Mean of all the Observations				29·814	59·8	48·85	53·5	21·5	34·75	339	207	263·92‡	7·43	10·025
Maximum of all the Observations				30·1	66	52	80	35	48	530	250	300·5	13·5	15
Minimum of all the Observations				29·2	54	43	40	5	28	280	80	194§	4·5	5
Difference between the Maximum and Minimum				0·9	12	9	40	30	20	250	170	106·5‖	9·0	10

* Falling since the morning. † Falling. ‡ Or, omitting obs. 3. as inexact, 266·96. § Or, omitting obs. 3., 230. ‖ Or, 70·5.

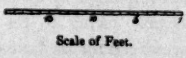
Largest quantity obtained being (300·5 × 1440 = 432,720) at the rate of 432,720 cubic inches, or, 250 cubic feet 72 cubic inches, in the 24 hours.

Smallest quantity, (194 × 1440 = 279,360) at the rate of 279,360 cubic inches, or 161 cubic feet 1152 cubic inches, in the 24 hours. Or, omitting obs. 3, (230 × 1440 = 331,200) at the rate of 331,200 cubic inches, or, 191 cubic feet 1152 cubic inches, in the 24 hours.

Mean quantity, (263,92 × 1440 = 380,044) at the rate of 380,044 cubic inches, or, 219 cubic feet 1612 cubic inches, in the 24 hours. Or, omitting obs. 3, (266·96 × 1440 = 384,422) at the rate of 384,442 cubic inches, or, 222 cubic feet 806 cubic inches, in the 24 hours.



Ground Plan of the King's Bath.



Description.

a. Area, inclosed by the hand-rail, twenty feet in diameter.
b. Upright bars supporting the hand-rail.
c. Pillars supporting the roof of the covered portion of the bath.
d. Steps leading down into the bath.
Apertures in the bath through which the water and gas rise.

..... Portions of the sides of the bath which are under cover.

e. Drains through which the water is allowed to discharge itself.

f. Stone cylinder eighteen inches in diameter occupying the centre of the bath, through which the spring principally rises.

APPENDIX.

Received January 22,—Read January 23, 1834.

Subsequently to the reading of the above Paper, I have had an opportunity of examining two tepid springs, which, since the setting in of the wet weather, have broken out at the foot of St. Vincent's Rocks, Clifton, immediately below the cliff from which the Suspension Bridge over the Avon was designed to spring, and on the summit of which the Observatory is erected.

The temperature of the one is 72°, that of the other 66° of Fahrenheit; and both are continually emitting bubbles of gas, which I found to consist of

Carbonic acid	3
Oxygen	8
Nitrogen	92
	103

The circumstance may be worth noticing on two accounts. First, as evincing that the cause, whatever it may be, of the heat, is not limited to a single spot, but is diffused over a considerable space in the direction along which the chasm extends; thus confirming an observation made to me by that intelligent naturalist, the late Mr. Miller of Bristol, that when, in consequence of rain, a new spring appears at the foot of these cliffs, its temperature is generally higher than that of the ordinary springs of the district: and secondly, as adding one additional instance to the catalogue of those thermal waters, which are accompanied by an evolution of nitrogen gas.

This is the more important, because, owing to the circumstance of the principal tepid spring at Bristol, which is preserved for medicinal purposes, being covered over at its source, it was impossible to ascertain from this one, whether the thermal waters of this place agreed in that particular with those of other localities, or whether they constituted an exception to the generality of this rule.

January 22nd, 1834.

II. *On the Empirical Laws of the Tides in the Port of London; with some Reflexions on the Theory.* *By the Rev.* WILLIAM WHEWELL, *A.M., F.R.S., Fellow and Tutor of Trinity College, Cambridge.*

Received November 13, 1833,—Read January 9, 1834.

THE present state of our knowledge of the tides is remarkably at variance with the complete and scientific character which Physical Astronomy is, in common opinion, supposed to have attained. We may, perhaps, most easily figure to ourselves the real condition of this subject, by imagining what the condition of other branches of astronomy would be, if some great natural or moral convulsion should sweep away our existing science, and replunge us in the ignorance of the dark ages, leaving extant only a few general notions concerning the theories which are at present established. In such a state of things, we may suppose that some tradition of the doctrine of universal gravitation would survive the change, and that learned men would still go on asserting that the various astronomical phenomena of the universe were owing to that cause; but the resources of mathematical art being, for the time, lost, they would be unable to prove the truth of such assertions: and, both the collected stores of observation, and the habit and apparatus of observing, being, in such a case, supposed to be annihilated, it would be long before there would arise persons able and willing to supply such deficiency; the more so as those who might make such collections would have still to seek for the mode of turning them to any use. If, in this state of things, a few persons should, by their own sagacity and labour, or by the aid of some traditionary secret, attain to the power of predicting phenomena with tolerable correctness, we may imagine that they would use their peculiar skill for purposes of gain, and that they would not readily admit the world at large to the knowledge of the secret which gave them a superiority over the rest of their countrymen.

Our knowledge of the tides, at the present time, exactly realizes this imaginary condition which we have supposed for astronomy in general. Our philosophers assert, without hesitation, that this phenomenon is the result of the law of the universal gravitation of matter; yet no one has hitherto deduced, from this law, the laws by which the phenomena are actually regulated with regard to time and place. Analysis has been largely used; but it has been employed only to deduce the consequences of certain assumed suppositions, which suppositions are acknowledged to be utterly different from the real state of the case: and where is the immediate advantage, for the purposes of sound philosophy, of analysis which does not solve the problem proposed, over no analysis at all? Some observations of the tides have no doubt been made, and more are now making; but it is not too much to say, that these are only a commencement

of the collections which the subject will require, to place it on a par with the other provinces of physical astronomy. The laws which connect the course of the observed tides with the motions and distances of the sun and moon are not known for any single port; and the tables, which in every other province of physics are the result of the knowledge which our men of science have accumulated for us, are, in this department, published by persons possessing and professing no theoretical views on the subject; and the methods by which they are calculated are not only not a portion of our published knowledge, but are guarded as secrets, and handed down as private property from one generation to another*.

Of course it cannot be intended here to speak with any disrespect of the persons who have calculated tide tables under these circumstances. Their labours are useful to the community in proportion as their tables are exact, which some of them are to a very remarkable degree. And, as no one thinks of condemning other persons who make a profit of any peculiar and secret knowledge which they may possess connected with any of the useful arts, there would be no justice in blaming those who do the same with respect to secrets which concern one of the most important arts, namely, navigation. But the circumstance most worthy of remark is, that there *should be* secrets in such a matter; that on such a subject our men of science should be ignorant of, and unable to discover, that which persons of much less elevated pretensions know and apply; that the laws which are to be collected either by the observation of facts, or by the deductions of theory, should not be known to our philosophers by either method, and yet should be in the possession of other persons, to a considerable extent. This circumstance makes our knowledge of the tides assume the character rather of a mere practical art, than of a portion of that complete and perfect science of which the other consequences of the law of universal gravitation supply examples.

Some persons may conceive that, in what has been said, I am disparaging too much the labours of the great mathematicians, Newton, Bernoulli, Laplace and others, who have employed their skill on this subject. But this opinion cannot, I conceive, be maintained with justice. It is well known that all the mathematical solutions of the problem have confessedly gone upon suppositions very remote from the real facts: Newton and Bernoulli, for instance, have assumed the form of the fluid spheroid, under the influence of the sun and moon, to be the form of equilibrium: Laplace has supposed the whole globe to be covered with water of an uniform depth. It is in no degree clear, that investigations conducted on such assumptions will give us even an approximation to the true result; and the only way in which the assumptions could be justified, would be by our finding, from observation, that the laws of the facts are such, or nearly such, as these hypothetical calculations give. If this agreement were

* What is here asserted was strictly true till the publication of Mr. Lubbock's Memoir on the Tides of the Port of London, and his Tide Tables, founded on his discussion of these. At present his Tide Tables are calculated by published methods; but the laws which these methods imply have not yet been compared with theory.

established, it would then, no doubt, become highly probable that the simplifications hypothetically introduced into the natural state of things were not such as materially to alter the general course of the phenomena.

But this has not been done by any of the theoretical writers above referred to. Undoubtedly most of them have undertaken to show that *some* of the known laws of the facts are accounted for by the theory, and that the measures of *some* of the phenomena agree with those which theoretical calculations give. But this has been executed only with respect to a few of the circumstances of the case. It has not been shown, by any writer, that the *general course* of the effects produced upon the tides, by the changes of position and distance of the heavenly bodies, is such as, according to the mathematical reasoning, it ought to be. In short, the mathematicians who have treated this subject have not completed their task by giving rules for the calculation of tide tables, and showing that the tables so produced agree with the general course of the observations in all essential circumstances.

The task just mentioned would consist of two parts; the theoretical deduction of the effects produced in the tides by changes of distance and position of the sun and moon; and the examination of the laws which such changes appear to follow in the observations; with a comparison of the two sets of results. The latter part of the task had not been executed, so far as I am aware, by any one, previously to Mr. Lubbock's discussion of the Tides of the Port of London, inserted in the Philosophical Transactions for 1831; and that memoir is hitherto the only published record of such an examination. The establishment, on theoretical grounds, of rules for the calculation of tide tables, has been attempted by Bernoulli and by Laplace. The methods recommended by the former are probably the foundation of those at present used by the calculators of such tables. The method of Laplace is complicated, and would be very laborious in practice. He has unfortunately, as appears to me, not put his process in such a form as to give a principal term, with smaller corrections for declination, parallax, and other circumstances if necessary, to be combined with the principal term. When the results of such an investigation are not made to assume this shape, the comparison of the formula with observation becomes a work of very repulsive labour and trouble.

It has already been stated, that some of the published tide tables are found to be not very incorrect when compared with observation. If any tide tables were so good that they might be considered as representing the general laws of the actual phenomena, we might discuss such tables, and compare them with theory, in the same manner as if they were the records of observation; and with this additional advantage, that they would be free from the effect of the accidental causes, as wind and other circumstances, which produce irregularities in the actual tide. Nor would it be difficult, by such a discussion, to discover the rules which are followed in the construction of such tables.

It may, however, be doubted whether there are any tables which are worth this trouble. Original tide tables are very few: I know of none except those which are published for Liverpool, and those for London. The former are remarkably exact; they

are calculated according to rules obtained by Mr. Holden, some years ago, from the examination of five years of observations made at the Liverpool Docks by Mr. Hutchinson, at that time harbour-master. The calculations are at present conducted by the Rev. George Holden, of Maghull, a descendant of the person who first invented the rules. Other Liverpool tide tables are also calculated by Mr. Woffinden. Of London tides several, apparently independent, tables are annually published; and though the differences of these are considerable, I do not know that any one set is considered as possessing a decided superiority in the general result. I am not aware that any tide tables are published for Brest, though so large a collection of observations has been made at that port, and though so much labour has been employed in the discussion of these, for the purpose of comparing certain points of Laplace's theory with them: nor have, I believe, tide tables for any place been calculated according to the method recommended in the Mécanique Celeste.

The method generally practised in England for the construction of tide tables for other places has been, to take the time which is stated in the London or the Liverpool tables, and, if necessary, to add or subtract some constant quantity, according to the place. The Liverpool tide tables are in this manner used, generally without correction, for the whole of the north-western coast of England: and tables are published professing to give the hours at most of the principal ports of England, in parallel columns; the hours at different places having constant differences. Thus the hour of high water at Plymouth is stated as always $1^h\ 55^m$ later than the hour in the same half-day at London. This assumption of a constant difference in the hours of high water at different places is, however, inexact; as we should expect it to be from considering the mode in which the tide is transmitted from one place to another, and as it appears to be from observation.

It appears, therefore, that the most promising mode of advancing our knowledge of the tides, is to examine the laws which can be collected from observation, taking so great a number of observations, that the effects of all accidental causes may disappear in the average results. The collection of observations discussed by Mr. Dessiou, under the direction of Mr. Lubbock, affords us an admirable opportunity for this examination; the collection including 13073 observations, and a period of nineteen years, from January 1st, 1808, to December 31st, 1826. Our object in this examination being to ascertain the manner in which the positions and distances of the heavenly bodies affect the time and height of high water, the mode of proceeding must be to examine how these two quantities depend upon the right ascension, declination and parallax of the sun and moon, and upon other astronomical elements, if such are found to be needed. The mean time of high water will be found to be affected by inequalities, depending on the elements just mentioned; and the law and amount of these inequalities may be collected from observations, without any reference to theory, (provided the observations are sufficiently numerous and their circumstances sufficiently varied,) in the same manner in which the greater inequalities of the moon, the variation, evection and annual equation, were detected by observation, long before the motions of the heavenly

bodies were referred to their true causes. Indeed, I believe the instances are comparatively few in the history of philosophy, in which the general laws of the phenomena have been pointed out by the theory before they had been gathered by observation. The laws of the tides, thus empirically obtained, may be used either as tests of the extant theories, or as suggestions for the improvement of those portions of mathematical hydraulics on which the true theory must depend. And this is the way in which we are most likely to discover how the theory must be applied. The problems regarding the motion of fluids, which we are unable to solve directly, are far too numerous to allow us to be surprised that we should be obliged to desert the *à priori* road in this case. The phenomena of waves, the motions of water in tubes, in canals, in rivers, the motion of winds, the resistance of fluids to bodies in motion, are all cases in which we are yet far from having drawn our analytical mechanics into a coincidence with experiment, or even a tolerable proximity to it. The theoretical analysis of the tides is, at present, in an equally imperfect state. It is not at all improbable that, as in many other cases, this problem in the mechanism of the solar system (for such it is) may be found in the end less complex and difficult than similar problems concerning the motions of smaller masses; but the problem remains still to be solved, or at least it remains still to be shown that the solution has been approximated to. I shall therefore here proceed to examine the empirical laws of the tides of the port of London, as they appear from the records of the nineteen years of observations above mentioned.

Chap. I. *On the Empirical Laws of the Time of High Water.*

The point which I have first to determine is, the manner in which the time of high water is affected by the right ascensions, declinations, and parallaxes of the sun and moon. For this purpose I shall have to consider *the establishment, the semimenstrual inequality, the corrections for lunar parallax, lunar declination,* and *solar parallax and declination.*

1. *The Establishment.*—The vulgar establishment of any port is the interval of time by which the time of high water follows the moon's transit *on the day of the new and full moon.* But it is the *mean* value of this interval of time which we must here employ, in order to simplify our discussion. This is what Laplace calls the *fundamental hour* of the port: I have termed it, in a former paper on this subject, the *corrected establishment,* since it is the lunar hour of high water, freed from the semimenstrual inequality. Its value at the London Docks is $1^h\ 26^m$, by the mean of all the observations.

2. *The Semimenstrual Inequality.*—The interval of tide and moon's transit is affected by a considerable inequality, which goes through its period twice in the space of one month: it may be considered as depending upon the moon's distance from the sun in right ascension; or, which is the same thing, on the solar time of the moon's transit. It has been examined by Mr. Lubbock, and shown to agree, with remarkable exactness, with the formula,

$$\tan 2\,(\theta' - \lambda') = -\frac{h \sin 2\,(\varphi - \alpha)}{k' + h \cos 2\,(\varphi - \alpha)},$$

in which λ' is the mean interval of the tide and transit, and θ the correct interval; ϕ the solar time of the moon's transit, and α a constant quantity. The ratio of the quantity h' to h is 2·9884 : 1; the quantity α is 2 hours.

According either to the method of Bernoulli or to that of Laplace, there would result from the theory an expression of the above form, for the interval of tide and moon's transit. By assuming suitably the values of $\frac{h}{h'}$ and α, the results of observation at other places may also be made to agree very closely with the above formula. The curves which represent by their ordinates the successive values of the above formula, when constructed for different places, exhibit a remarkable general similarity, as may be seen in the Philosophical Transactions, 1831, where Mr. Lubbock has given these curves for Portsmouth, Plymouth, Sheerness, London and Brest. The curve is symmetrical with respect to the axis, intersecting it when $\varphi = \alpha$, and when $\varphi = \alpha +$ a fourth of a circumference. Its ordinate has a negative minimum and a positive maximum, which are equal in magnitude; but these values are not midway between the values 0, consequently the ordinate increases more rapidly after the minimum and before the maximum, than it diminishes before the minimum and after the maximum. This property appears very clearly in the curves constructed for all the above ports.

But in other respects the result of the observations, thus compared, does not agree with the theory. According to the theory, the quantities h and h' express the amount of the separate solar and lunar tides respectively, and as the ratio of these effects must be the same for all places, the maximum value of the semimenstrual inequality ought to be the same in all the above cases; namely, the time corresponding to half the angle whose tangent is $\frac{h}{\sqrt{h'^2 - h^2}}$. If, as Laplace finds from the Brest observations, $\frac{h'}{h} = 2{\cdot}6157$, the angle corresponding to the above tangent is 22° 28′; the maximum value of the inequality is 45^m, and the double of this, or 1^h 30^m, is the difference of the greatest and least interval of the tide and moon's transit.

According to observation, the difference of the greatest and least intervals is as follows*:

London	1^h 28^m
Sheerness	1 29
Portsmouth	1 21
Plymouth	1 36
Brest	1 19

It appears unlikely that the difference in these values for Plymouth and Brest, or even Plymouth and Portsmouth, can depend upon accidental causes, or too limited a number of observations. It would appear, therefore, that the *coefficient of the semimenstrual inequality*, $\left(\frac{h}{h'}\right)$, is different at different places; a circumstance which no extant theory would have led us to expect. This subject, however, deserves further

* We suppose here the effects of parallax and declination to be eliminated by the averages of the observations.

examination; and it would be important for this, as well as for other purposes, to discuss some large collection of observations for other places than London, in the mode which Mr. LUBBOCK has applied to the London observations. Such collections are known to exist for Brest and for Liverpool.

The quantity α in the formula is undoubtedly different at different places. It is what Mr. LUBBOCK, following LAPLACE, calls the *retard*, and depends upon what I have termed the *age of the tide*. It cannot be determined with certainty or exactness without the use of a large body of observations. Its value at London is 2^h, at Brest $1^h\,12^m$; at Portsmouth it is intermediate between the value at Brest and at London, as we should expect, being about $1^h\,30^m$; but at Plymouth it is greater than it is at London, which, as Mr. LUBBOCK observes, is at present a very inexplicable circumstance; probably to be explained only by the determination of the value of this quantity for several other places.

3. *The Correction for Lunar Parallax.*—Mr. LUBBOCK has classified the tide observations which he has discussed according to the value of the moon's horizontal parallax which existed at the time when the tide occurred, and also according to the hour of the moon's transit, so as to form a table of double entry of the differences from the mean interval: this is Table XVII. in his Memoir of 1831, which I here insert.

TABLE showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every Minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 0	+12	+ 9	+ 4	....	− 3	− 4	−13	−14
0 30	+12	+ 9	+ 2	+ 2	− 3	− 5	− 9	−11
1 0	+10	+ 8	+ 3	+ 5	− 1	− 4	− 9	−11
1 30	+ 8	+ 5	+ 3	+ 5	− 1	− 3	−10	−11
2 0	+ 8	+ 6	+ 2	+ 3	+ 1	− 1	− 8	− 9
2 30	+ 7	+ 5	+ 1	+ 1	+ 2	− 2	− 6	− 8
3 0	+ 6	+ 4	+ 2	....	+ 2	− 2	− 6	
3 30	+ 6	+ 4	+ 3	+ 1	+ 3	− 2	− 5	
4 0	+ 4	+ 3	+ 2	− 1	+ 2	− 2	− 6	
4 30	....	+ 1	+ 3	− 1	− 1	− 2	− 8	
5 0	+ 1	+ 1	+ 3	+ 1	0	− 1		
5 30	+ 1	0	− 1	+ 2	− 1	− 1		
6 0	+ 1	+ 1	− 3	+ 1	− 2	− 2		
6 30	+ 2	+ 4	− 3	− 1	− 3	− 3		
7 0	+ 4	+ 2	− 3	− 2	− 5	− 4		
7 30	+ 9	− 2	− 2	− 4	− 7	− 7	− 7	
8 0	+16	0	0	− 3	− 8	− 6	−11	
8 30	+21	+ 8	+ 3	+ 1	− 7	− 6	−12	
9 0	+19	+ 9	+ 4	+ 1	− 9	−10	−16	
9 30	+17	+11	+ 7	+ 1	− 8	−11	−18	
10 0	+16	+12	+ 8	0	− 5	−12	−17	
10 30	+15	+13	+ 8	− 1	− 2	−10	−14	−17
11 0	+13	+12	+ 7	− 2	− 2	− 8	−14	−18
11 30	+13	+10	+ 6	− 1	− 2	− 4	−16	−16

On examining this Table, it appears that in the column corresponding to H. P. 57′, the differences from the mean, or corrections for parallax, are very small for all hours of the moon's transit, (ranging from + 5^m to − 4^m,) and that the positive nearly balance the negative values. We may suppose, therefore, that for H. P. 57′ nearly, the correction is 0. It appears also that the correction is generally negative when the H. P. is greater than 57′, and positive when it is less, the exceptions being of small amount compared with the general mass of observations; and if we take the sums in each vertical column of Table XVII. we shall find that they are nearly as the difference of the parallax from the mean value 57′. It appears, therefore, that this correction must involve a factor (P − p), when P is the mean horizontal parallax of the moon (or 57′), and p any other value of her horizontal parallax.

If we take any vertical column of this Table, and thus follow the correction through the various hours of the moon's transit, we find that for all values of the parallax the correction is very small, when the moon passes at $5^h\ 30^m$ or 6^h, and that the positive and negative values in that case nearly balance each other. In each column, when the hour of transit is either greater or less than this, the correction increases with the difference of hour, and proceeds to a maximum, which appears to occur about 9^h or 10^h transit. As a simple way of satisfying these conditions, we may suppose the correction to involve the factor $\sin^2(\varphi - \beta)$ when β is a constant quantity: and combining this factor with the one already found, we shall have B (P − p) $\sin^2(\varphi - \beta)$ for this correction in minutes of time.

It appears that in order to give the maximum value of this correction when it occurs at about 10^h, β must not be much different from 4^h. In order to determine B, take the formula B (P − p) $\sin^2(\varphi - \beta)$ for every half-hour: its value is

$$2\,B\,(P-p)\,\{\sin^2 7\tfrac{1}{2}^\circ + \sin^2 15^\circ + \sin^2 22\tfrac{1}{2}^\circ + \sin^2 30^\circ + \sin^2 37\tfrac{1}{2}^\circ + \sin^2 45^\circ + \sin^2 82\tfrac{1}{2}^\circ + \sin^2 75^\circ + \sin^2 67\tfrac{1}{2}^\circ + \sin^2 60^\circ + \sin^2 52\tfrac{1}{2}^\circ\}$$
$$= 11\,B\,(P-p).$$

Comparing this with the sums for H. P. 54′, 55′, 56′, 57′, 58′, 59′, (the other columns being incomplete,) we have,

Horizontal parallax..	54′	55′	56′	57′	58′	59′
Formula	33 B	22 B	11 B	0	−11 B	− 22B
Observed sums	221	135	59	8	−60	−112

Hence, taking the sums, 99 B = 595, whence B = 6, and the expression is 6 (P − p) $\sin^2(\theta - 4^h)$.

This may be put in the form 3 (P − p) $\left(1 - \cos 2\,(\theta - 4^h)\right)$,

or
$$3\,(P-p)\left(1 + \sin 2\,(\theta - 1^h)\right).$$

The agreement with the sums observed is as follows:

Horizontal parallax..	54′	55′	56′	57′	58′	59′
Formula	198	132	66	0	−66	−132
Observed sums.....	221	135	59	8	−60	−112

And on applying this correction, the residual quantities are, with one or two exceptions, within the limits $+ 4^m$ and $- 4^m$.

4. *The Correction for Lunar Declination.*—In Table IX. of his Memoir, Mr. Lubbock has arranged the intervals of the time of tide and moon's transit according to the declination of the moon, taken for every three degrees; and in Table XIX. he has given the difference of these intervals from the mean, arranged according to declination and time of transit. On inspecting this Table, it appears that this difference from the mean, or correction for lunar declination, is, for all values of the time of transit, 0 when the declination has its mean value of about 16°, positive when the declination is less, and negative when it is greater than this. The correction for a given declination, as shown in the vertical columns, is not constant, but it appears difficult to determine whether the variations are accidental or are the consequences of the form of the correction. Till we have better data, I will neglect these variations.

Taking, then, the sums of the vertical columns in Table XIX., we find as follows (the sums being expressed in minutes):

Table showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every Three Degrees of the Moon's Declination.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 0	+ 8	+ 5	+ 7	+ 5	+ 2	+ 2	− 3	− 6	−11	−10
0 30	+ 9	+ 6	+ 9	+ 7	− 2	....	− 2	− 4	− 7	−11
1 0	+ 8	+ 8	+10	+ 5	+ 2	+ 2	− 3	− 5	− 6	−11
1 30	+ 5	+ 8	+ 7	+ 2	+ 4	+ 3	− 4	− 7	− 6	−12
2 0	+ 6	+ 8	+ 6	+ 4	+ 3	+ 3	− 2	− 5	− 5	−10
2 30	+ 6	+ 8	+ 5	+ 7	+ 2	+ 2	− 1	− 4	− 5	− 9
3 0	+ 9	+ 9	+ 6	+ 7	+ 4	+ 2	− 2	− 7	− 7	−11
3 30	+11	+11	+ 9	+ 7	+ 5	+ 2	− 2	− 9	−10	−13
4 0	+ 9	+10	+10	+ 7	+ 7	+ 1	0	− 7	−10	−16
4 30	+ 8	+ 8	+ 8	+ 8	+ 8	− 1	0	− 6	−11	−18
5 0	+13	+12	+11	+12	+ 9	0	0	− 4	− 9	−16
5 30	+17	+14	+12	+13	+ 6	0	− 3	− 5	−10	−14
6 0	+20	+16	+13	+13	+ 6	+ 1	− 5	− 7	−13	−17
6 30	+21	+19	+12	+13	+ 8	....	− 7	− 9	−18	−21
7 0	+21	+19	+12	+17	+10	+ 4	− 6	− 8	−19	−27
7 30	+16	+16	+14	+18	+10	+ 5	− 8	− 6	−20	−34
8 0	+14	+18	+16	+16	+ 8	+ 4	− 6	− 6	−23	−30
8 30	+16	+15	+15	+12	+ 8	+ 2	− 1	− 5	−24	−22
9 0	+13	+ 9	+12	+ 7	+ 7	− 2	− 5	− 8	−18	−17
9 30	+13	+ 7	+11	+ 6	+ 7	− 2	− 6	− 7	− 7	−12
10 0	+11	+ 6	+ 6	+ 6	+ 4	− 2	− 8	− 7	− 5	−13
10 30	+11	+ 8	+ 3	+ 6	+ 3	0	− 7	− 5	− 5	−13
11 0	+ 9	+ 7	+ 3	+ 4	+ 3	+ 1	− 6	− 6	− 9	−13
11 30	+ 8	+ 3	+ 4	+ 3	+ 4	+ 3	− 3	− 8	−14	−12
Sums	+282	+250	+221	+205	+128	+30	−90	−151	−272	−382

It is tolerably manifest that these sums decrease faster for the large declinations than for the small ones; and we shall probably see the law more clearly by referring the correction to declination 0° than to the mean declination. For this purpose subtract from each sum the correction for declination 0°, and we have

Declination..	0	3°	6°	9°	12°	15°	18°	21°	24°	27°
Corrected sum	0	−32	−61	−77	−154	−252	−372	−433	−554	−664

It appears that the numbers here are not very remote from the ratio of the squares of the sines of the declinations. In fact, if we take the formula $-3168 \sin^2 \delta$, we have

For declination	0	3°	6°	9°	12°	15°	18°	21°	24°	27°
Formula	0	−9	−34	−77	−138	−229	−303	−408	−525	−654

which may pass for a first approximation to the observed result, when it is considered how much the errors are increased by addition. Dividing by 24, this gives $-132^m \sin^2 \delta$ for the correction to be applied to each result calculated for declination 0. When δ is about 16°, the mean value, this correction is 11^m. Hence $11^m - 132^m \sin^2 \delta$ is the correction to be applied to the mean value. This gives us the following Table, which is a first approximation to Table XIX. of Mr. LUBBOCK.

For declination ..	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction	+ 11	+ 10	+ 9	+ 8	+ 5	+ 2	− 2	− 6	− 11	− 16
The sums.....	+264	+240	+216	+192	+120	+48	−48	−144	−264	−384

Which agree nearly with

+282 +250 +221 +205 +128 +30 −90 −151 −272 −382

the observed results, with sufficient accuracy.

We may observe that the expression $11 - 132 \sin^2 \delta$ is 0 when $\sin^2 \delta = \frac{1}{12}$ or $\delta =$ 16° 45′. This is the mean value of Δ, because the correction is applied to the mean. Therefore the expression is $132 (\sin^2 \Delta - \sin^2 \delta)$.

In each vertical column of Table XIX., the value appears to be greatest and least when the time of the moon's transit is about 7^h and 1^h. Hence we shall take the correction given by the above formula, and try whether the *residual phenomenon*, after this correction has been applied, is governed by any fixed rule.

For this purpose apply the above correction with an opposite sign to Mr. LUBBOCK's Table XIX. The numbers in the columns are minutes.

TABLE XIX. freed from the Term $11 - 132 \sin^2 \delta = 132 (\sin^2 \Delta - \sin^2 \delta)$.

Decl.	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°	Sums.	
Corr.	−11	−10	− 9	− 8	− 5	− 2	+ 2	+ 6	+11	+16		
☽'s Transit.											0° to 15°.	18° to 27°.
h m												
0 0	− 3	− 5	− 2	− 3	− 3	0	− 1	0	0	+ 6	−16	+ 5
0 30	− 2	− 4	0	− 1	− 7	....	0	+ 2	+ 4	+ 5	−14	+11
1 0	− 3	− 2	+ 1	− 3	− 3	0	− 1	+ 1	+ 5	+ 5	−10	+10
1 30	− 6	− 2	− 2	− 6	− 1	+ 1	− 2	− 1	+ 5	+ 4	−16	+ 6
2 0	− 5	− 2	− 3	− 4	− 2	+ 1	0	+ 1	+ 6	+ 6	−12	+13
2 30	− 5	− 2	− 4	− 1	− 3	0	+ 1	+ 2	+ 6	+ 7	−15	+16
3 0	− 2	− 1	− 3	− 1	− 1	0	0	− 1	+ 4	+ 5	− 8	+ 8
3 30	0	+ 1	0	− 1	0	0	0	− 3	+ 1	+ 3	+ 0	+ 1
4 0	− 2	0	+ 1	− 1	+ 2	− 1	+ 2	− 1	+ 1	0	− 1	+ 2
4 30	− 3	− 2	− 1	0	+ 3	− 3	+ 2	0	0	− 2	− 6	0
5 0	+ 2	+ 2	+ 2	+ 4	+ 4	− 2	+ 2	+ 2	+ 2	0	+12	+ 6
5 30	+ 6	+ 4	+ 3	+ 5	+ 1	− 2	− 1	+ 1	+ 1	+ 2	+17	+ 3
6 0	+ 9	+ 6	+ 4	+ 4	+ 1	− 1	− 3	− 1	− 2	− 1	+23	− 7
6 30	+10	+ 9	+ 3	+ 4	+ 2	....	− 5	− 3	− 7	− 5	+29	−20
7 0	+10	+ 9	+ 3	+ 6	+ 5	+ 2	− 4	− 2	− 8	−11	+35	−25
7 30	+ 5	+ 6	+ 5	+ 7	+ 5	+ 3	− 6	− 0	− 9	−18	+31	−33
8 0	+ 3	+ 8	+ 7	+ 5	+ 3	+ 2	− 4	− 0	−12	−14	+26	−30
8 30	+ 5	+ 5	+ 6	+ 1	+ 3	0	+ 1	+ 1	−13	− 6	+20	−17
9 0	+ 2	− 1	+ 3	− 1	+ 2	− 4	− 3	− 2	− 7	− 1	+ 1	−13
9 30	+ 2	− 3	+ 2	− 2	+ 2	− 4	− 4	− 1	+ 4	+ 4	− 3	− 5
10 0	0	− 4	− 3	− 2	− 1	− 4	− 6	− 1	+ 6	+ 3	−14	+ 2
10 30	0	− 2	− 6	− 2	− 2	− 2	− 5	+ 1	+ 6	+ 3	−14	+ 5
11 0	− 2	− 3	− 6	− 4	− 2	− 1	− 4	0	+ 2	+ 3	−20	− 1
11 30	− 3	− 7	− 5	− 5	− 1	+ 1	− 1	− 2	− 3	+ 4	−20	− 2

From the changes of magnitude and sign, it appears that each vertical column of differences may be represented nearly by a term $A \sin 2 (\varphi - \gamma)$, φ being the hour-angle of the first column, and γ a certain other angle. Also it appears that in each horizontal line, A passes from positive to negative, and *vice versâ*, when the declination passes through its mean value. Hence there is a factor $\delta - \Delta$, Δ being the mean value of the declination.

For declinations less than the mean, the maximum values of the correction are about the hours of transit $0^h\ 0^m$ and $7^h\ 0^m$. This would give for γ the value $3^h\ 30^m$.

For declinations greater than the mean, the maximum values of the correction would occur nearly when the hour of transit is $1^h\ 30^m$ or $7^h\ 30^m$. This would give for γ, $4^h\ 30^m$; the mean of this and the other value is 4^h.

Hence the formula for the above residual quantities will be

$$D (\Delta - \delta) \sin 2 (\varphi - \gamma);$$

where, however, instead of $\Delta - \delta$, we may have other functions, as $\sin \Delta - \sin \delta$, $\sin^2 \Delta - \sin^2 \delta$. Hence the whole correction for lunar declination appears to be

$$132 (\sin^2 \delta - \sin^2 \Delta) + D (\delta - \Delta) \sin 2 (\varphi - \gamma),$$

which will be simplified if we put $\sin^2 \delta - \sin^2 \Delta$ for $\delta - \Delta$; the expression then becomes,

$$(\sin^2\delta - \sin^2\Delta)\left(132 + \mathrm{D}\sin 2(\varphi - \gamma)\right).$$

We have to find D. For that purpose take the formula

$$(\sin^2\delta - \sin^2\Delta)\,\mathrm{D}\sin 2(\varphi - \gamma),$$

which expresses the residual phenomenon just given from Table XIX. Take the case where $\delta = 0$, and we have for the first vertical column, the expression

$$-\mathrm{D}\sin^2\Delta\sin 2(\varphi - \gamma).$$

The vertical column contains all the values of this for every half-hour of the value of $\varphi - \gamma$, that is, for values of $\sin 2(\varphi - \gamma)$ taken at intervals of 15° round the circumference. Taking the sum of these values for one semicircle, it is, by known formulæ,

$$\frac{\sin 90^\circ \times \sin 82\tfrac{1}{2}^\circ}{\sin 7\tfrac{1}{2}^\circ} = \tan 82\tfrac{1}{2}^\circ = 7{\cdot}5957.$$

Now this sum in the Table is 45 if we take the mean of the positive and negative values; observing, however, that this value compared with the succeeding columns appears to be smaller than the general course of the numbers would give it. Hence,

$$\mathrm{D}\sin^2\Delta \times 7{\cdot}5957 = 45\,;\quad \mathrm{D} = 72 \text{ nearly.}$$

Hence $\mathrm{D}\sin^2\Delta = 6$. But it will agree better with the general numbers to make $\mathrm{D} = 7$, and the expression for the residual phenomenon is

$$84(\sin^2\delta - \sin^2\Delta)\sin 2(\varphi - \gamma).$$

The values of $84(\sin^2\Delta - \sin^2\delta)$ for the successive values of δ are hence found; and hence the corrections.

Assuming $\gamma = 4^h$, the following Table represents the table of the residual phenomenon.

TABLE of the Expression $84(\sin^2\Delta - \sin^2\delta)\sin 2(\varphi - \gamma)$.

	$\delta =$	0°	3°	6°	9°	12°	15°	18°	21°	23°	27°
	$84(\sin^2\Delta - \sin^2\delta) =$	7	7	6	5	3	1	0	− 4	− 7	−10
φ	$\sin 2(\varphi - \gamma)$.										
1^h	−1·000	− 7	− 7	− 6	− 5	− 3	− 1	0	+ 4	+ 7	+10
2	−0·866	− 6	− 6	− 5	− 4	− 3	− 1	0	+ 3	+ 6	+ 9
3	−0·100	− 4	− 4	− 4	− 3	− 2	− 1	0	+ 2	+ 4	+ 5
4	0·000	0	0	0	0	0	0	0	0	0	0
5	0·500	+ 4	+ 4	+ 4	+ 3	+ 2	+ 1	0	− 2	− 4	− 5
6	0·866	+ 6	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9
7	1·000	+ 7	+ 7	+ 6	+ 5	+ 3	+ 1	0	− 4	− 7	−10
8	0·866	+ 6	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9
9	0·500	+ 4	+ 4	+ 4	+ 3	+ 2	+ 1	0	− 2	− 4	− 5
10	·000	0	0	0	0	0	0	0	0	0	0
11	−0·500	− 4	− 4	− 4	− 3	− 2	− 1	0	+ 2	+ 4	+ 5
12	−0·866	− 6	− 6	− 5	− 4	− 3	− 1	0	+ 3	+ 6	+ 9

This agrees as to its changes of magnitude and sign, and as to the mean of the numbers, with the table of the residual quantities, p. 25. The formula,

$$(\sin^2\delta - \sin^2\Delta)\{132 + 84\sin 2(\varphi - 4^h)\}$$

will therefore express, with considerable accuracy, the general course and average values of the numbers in Mr. Lubbock's Table XIX.

But if the correction, instead of being applied to the *mean* value of the interval (of tide and transit), had been applied to the interval calculated for declination 0, it is clear the correction would have been

$$\{132 + 84 \sin 2(\phi - 4^h)\} \sin^2 \delta.$$

Mr. Lubbock has given other tables also, from which the mean correction for lunar declination may be collected. His Table XV., which contains the differences of the intervals of the time of moon's transit and high water from the mean interval, arranged according to the calendar months and to times of the moon's transit, is in fact principally a table of the correction for lunar declination. For by examination of that table, it will be seen that the correction in each month goes through its cycle of 0, +, 0, −, in one semirevolution of the moon; that is, while the declination passes from its maximum north, to its maximum south, value: and since these results are the mean of nineteen years, the moon will have been nearly as much on the north as on the south of the ecliptic, and the result will be nearly the same as if she had moved in the ecliptic. It may be observed, however, that it appears by what has been shown above, that the corrections increase faster than the declinations; and therefore the corrections due to the high declinations will not be quite balanced by those due to the declinations which correspond to an equal opposite celestial latitude.

It is to be noticed, also, that this Table XV., being arranged for calendar months, contains the effect of solar declination and parallax as well as of lunar declination. It also contains the effect of the equation of time; the times of the moon's transit being given in *mean* solar time, whereas we suppose the tide to depend on the hour-angle of the moon from the sun, that is, on the transit in true solar time. These effects may be eliminated, and the effect of the changes of lunar declination upon the tide-hour may be determined from this table in an approximate manner; but the accuracy of such a determination is necessarily less than that of the one already obtained, and I shall therefore not insert it here.

5. *The Solar Correction.*—The sums of the positive and of the negative numbers in each vertical column of Mr. Lubbock's Table XV. would be equal, if the inequality depended on the moon alone, since each column contains the corrections which occur in a half-revolution of the moon. Therefore the difference of these sums is due to a solar inequality, and the mean excess or defect must be subtracted or added in order to obtain the corrections due to the moon. These means are as follow:

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	
	+105 − 61	+ 48 −147	+ 63 −144	+64 −96	+90 −79	+120 − 71	+128 − 62	+110 − 68	+90 −90	+ 99 −111	+ 98 −120	+103 − 74	
	+ 44	− 99	− 81	−32	+11	+ 49	+ 66	+ 42	0	− 12	− 22	+ 29	
Means	+ 2	− 4	− 3	− 1	0	+ 2	+ 3	+ 2	0	− 1	− 1	+ 1	Solar inequality.

It appears that this correction changes from positive to negative four times in the course of the year, and hence may be approximately represented by $m \sin 2(\theta - \mu)$ when θ is the sun's right ascension. But the maximum and minimum values in different parts of the year are of unequal magnitude and at unequal intervals. This may be reconciled with an expression of the form $m \sin 2(\theta - \mu) + n \sin(\theta - \nu)$; and we might determine m, n, μ, ν, so as to make the expression agree nearly with the result of observation. In fact, however, this would not be worth while, except we had the empirical law confirmed by the results of observations at other places; for the greatest values of this correction are -4^m and $+3^m$. We here exclude the effects of the equation of time.

It is not difficult to see why the solar correction assumes such a form as this. It includes the corrections due both to the sun's declination and his parallax. The former effect is twice a minimum and twice a maximum in the course of a year; the latter once only. The two effects are not immediately separated in the tables, because the sun's perigee being nearly stationary, the cycle of changes due to solar parallax and the double cycle of changes due to solar declination coincide.

When the form and amount of the solar correction are more exactly determined, it may be more exactly compared with the theory.

Chap. II. *On the Empirical Laws of the Height of High Water.*

The same kind of discussion of the observations which has enabled us to obtain approximately the laws of the *times* of high water, will also give similar information with respect to the *heights*, since these have been observed at the docks, and the results tabulated by Mr. Lubbock, in the same way as the others. The heights will be affected in the same way as the tides, by a *semimenstrual inequality*, by corrections for *lunar parallax and declination*, and by a *solar correction*.

1. *Of the Mean Level of the Water.*—The quantities which are wanted for the comparison of observed heights with the theory, are the total height of the tide, that is, the difference of high and low water. The heights of low water are not given in the London observations, and we have, therefore, only the differences of the high waters to reduce to their laws.

A comparison of these with the theory, supposes the mean level of the water to be constant, that is, the mean of the heights of high and low water to be the same, whatever be the height of the tide. I do not know whether this permanency of the mean level has been verified at the London Docks. It has been ascertained to be true in several other cases, and is probably universal, or at least liable to few and peculiar exceptions.

This mean level may be determined by the mean of many observations, and is a more fixed and distinct level than any level depending on a smaller number of observations. It is, moreover, free from the irregularities to which levels selected in any other way are exposed. Thus the level of high water, or of low water, at spring tides, or at neap tides, is different according to the different effects of lunar and solar parallax and declination.

I proceed to discuss the variations of the heights of the London tides.

2. *The Semimenstrual Inequality.*—The law of the heights is contained in the last column of Mr. LUBBOCK's Table V., which gives the heights for every half-hour, mean time, of the moon's transit, taking a mean of the months of the year.

In order to obtain a formula expressing this series of quantities, we observe that the maximum and minimum are nearly when the times of transit are 2^h and 8^h respectively; that the mean of the extreme heights is 21·1 feet, and the difference of the extremes 3·4 feet. Hence the height may be expressed approximately by the formula $21{\cdot}1 + 1{\cdot}7 \cos 2(\varphi - 30°)$. It appears, however, that the maximum and minimum occur a little earlier than 2^h and 8^h. I shall therefore assume for the height the formula $21{\cdot}1 + 1{\cdot}7 \cos(2\varphi - 51°)$; we shall then have the following comparison with observations.

φ		$\cos(2\varphi - 51)$	Height observed.	Height calculated.	Diff.	Diff. − ·23
h	m		Feet.	Feet.		
2	0	·9877	22·78	22·80	·02	− ·21
3	0	·7771	22·42	22·59	·17	− ·06
4	0	·3584	21·71	22·10	·39	+ ·16
5	0	− ·1564	20·83	21·28	·45	+ ·22
6	0	− ·6293	20·03	20·37	·34	+ ·21
7	0	− ·9335	19·51	19·56	·05	− ·18
8	0	− ·9877	19·42	19·43	·01	− ·22
9	0	− ·7771	19·78	20·10	·32	+ ·09
10	0	− ·3584	20·49	20·92	·43	+ ·20
11	0	·1564	21·37	21·85	·48	+ ·25
0	0	·6293	22·17	22·46	·29	+ ·06
1	0	·9335	22·69	22·72	·03	− ·20

It is evident that this is a first approximation. Also the difference, which is always positive, follows the law of a sine. To show this, subtract from this difference ·23, as is done in the last column.

The difference in the last column will be 0 when φ is $0^h\ 30^m$ and $6^h\ 30^m$ nearly, and will, in the course of 6 hours of φ, go through all the values of $\sin\varphi$. Hence it may be represented by $-c \sin(4\varphi - 30°)$, and it is evident that c is nearly ·23.

Comparison of the residual Phenomenon of the Semimenstrual Series of Heights, freed of the Terms $21{\cdot}1 + 1{\cdot}7 \cos(2\varphi - 51)$, with the Formula $·23 - ·23 \sin(4\varphi - 30)$.

φ	$\sin(4\varphi - 30)$	Form.	Obs.	Excess of Obs.
h	Feet.	Feet.	Feet.	Feet.
0	− ·5	·35	·29	−·06
1	+ ·5	·11	·03	−·08
2	+ 1·0	0	·02	+·02
3	+ ·5	·11	·17	+·06
4	− ·5	·35	·39	+·04
5	− 1·0	·46	·45	−·01
6	− ·5	·35	·34	−·01
7	+ ·5	·11	·05	−·06
8	+ 1·0	0	·01	+·01
9	+ ·5	·11	·32	+·21
10	− ·5	·35	·43	+·08
11	− 1·0	·46	·48	+·02

It appears from the nature of the still residual differences, that we might bring our formula still nearer to observation; but as the differences do not exceed $\frac{1}{10}$th of a foot, except in one instance, this exactness would be, in the present state of our knowledge, superfluous.

Hence the mean height in feet of the tide at London Dock is represented by

$$21{\cdot}1 + 1{\cdot}7 \cos(2\varphi - 51^\circ) + {\cdot}23 - {\cdot}23 \sin(4\varphi - 30^\circ),$$
$$\text{or } 21{\cdot}33 + 1{\cdot}7 \cos(2\varphi - 51^\circ) - {\cdot}23 \sin(4\varphi - 30^\circ)$$

where φ is the hour-angle of the moon's transit, mean time.

3. *Correction of the Heights for Lunar Parallax.*—Table XVIII. of Mr. Lubbock contains the effect of variations of the moon's distance.

Table showing the Difference in the Height of High Water, and the Mean Height for every Minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	−0·52	−·33	−·30	+·10	+·06	+·23	+ ·33	+·53
0 30	− ·50	−·28	−·20	+·08	+·17	+·41	+ ·36	+·53
1 0	− ·58	−·44	−·09	−·07	+·26	+·24	+ ·31	+·61
1 30	− ·55	−·46	+·03	−·07	+·23	+·23	+ ·41	+·77
2 0	− ·57	−·52	−·15	−·05	+·12	+·24	+ ·50	+·75
2 30	− ·54	−·45	−·28	+·10	+·08	+·34	+ ·69	+·79
3 0	− ·68	−·37	−·25	+·06	+·05	+·42	+ ·83	
3 30	− ·63	−·23	−·13	+·08	+·13	+·56	+1·00	
4 0	− ·57	−·28	−·12	+·14	+·20	+·66	+ ·98	
4 30	− ·49	−·29	−·13	+·23	+·67	+·76	+ ·83	
5 0	− ·50	−·40	−·30	+·25	+·53	+·80		
5 30	− ·47	−·40	−·37	+·37	+·50	+·86		
6 0	− ·45	−·28	−·19	+·27	+·50	+·77		
6 30	− ·54	−·21	−·09	+·05	+·40	+·62	+ ·43	
7 0	− ·64	−·29	−·11	+·05	+·31	+·54	+ ·53	
7 30	− ·75	−·48	−·28	+·01	+·15	+·33	+ ·58	
8 0	− ·68	−·30	−·25	+·05	+·02	−·08	+ ·54	
8 30	− ·54	−·27	−·26	+·01	−·20	+·42	+ ·44	
9 0	− ·26	−·23	−·21	−·11	−·23	−·76	+ ·39	
9 30	+ ·03	−·24	−·15	−·26	−·09	+·30	+ ·45	+·87
10 0	− ·09	−·30	−·11	−·14	+·14	+·36	+ ·49	+·69
10 30	− ·20	−·37	−·07	+·03	+·32	+·36	+ ·48	+·55
11 0	− ·31	−·31	−·14	+·21	+·26	+·26	+ ·51	+·61
11 30	− ·43	−·22	−·25	+·27	+·13	+·14	+ ·46	+·68

If we take the means of the vertical columns, they are, in hundredths of feet,

Horizontal parallax	54′	55′	56′	57′	58′	59′	60′	61′
Means	−47	−33	−18	+7	+20	+37	+47	+67

These are very nearly as the differences of the parallax. We shall find that the formula 1·7 (p − P) when p is the parallax, and P is 57′, will very nearly give this result. It gives, in fact,

−51 −34 −17 0 +17 +34 +51 +68

Hence, applying these corrections with opposite signs to Table XVIII., we obtain the residual phenomenon as follows, in hundredths of feet.

TABLE XVIII. freed from the Sum 1·7 (p − P).

Hor. Par..	54′	55′	56′	57′	58′	59′	60′	61′
Corr.	+51	+34	+17	0	−17	− 34	−51	−68
h m								
0 0	− 1	+ 1	−13	+10	−11	+ 11	−18	−15
0 30	+ 1	+ 6	− 3	+ 8	0	7	−15	−15
1 0	− 7	−10	+ 8	− 7	+ 9	− 10	−20	− 7
1 30	− 4	−12	+20	− 7	+ 6	− 11	−10	+ 9
2 0	− 6	−18	+ 2	− 5	− 5	− 10	− 1	+ 7
2 30	− 3	−11	−11	+10	− 9	− 11	+18	+11
3 0	−17	− 3	− 8	+ 6	−12	+ 8	+32	
3 30	−12	+11	+ 4	+ 8	− 4	+ 32	+49	
4 0	− 3	+ 6	+ 5	+14	+ 3	+ 32	+47	
4 30	+ 2	+ 5	+ 4	+23	+50	+ 42		
5 0	+ 1	− 6	−13	+25	+36	+ 46		
5 30	+ 4	− 6	−20	+37	+33	+ 52		
6 0	+ 6	+ 6	− 2	+27	+33	+ 43		
6 30	− 3	+13	+ 8	+ 5	+23	+ 28	− 8	
7 0	−13	+ 5	+ 6	+ 5	+14	+ 20	+ 2	
7 30	−24	−14	−11	+ 1	− 2	− 1	+ 7	
8 0	−14	+ 4	− 8	+ 5	−15	− 42	+ 3	
8 30	− 3	+ 7	− 9	+ 1	−37	+ 8	+ 7	
9 0	+26	+11	− 4	−11	−40	−110	−12	
9 30	+54	+10	+ 2	−26	−26	− 4	− 6	+19
10 0	+42	+ 4	+ 6	−14	− 3	+ 2	− 2	+ 1
10 30	+31	− 3	+10	+ 3	+15	+ 2	− 3	− 6
11 0	+20	+ 3	+ 3	+21	+ 9	− 6	0	− 7
11 30	+ 8	+12	− 8	+27	− 4	− 20	− 5	0

There is no very manifest rule in this Table. There appear to be many large positive terms between the hours $2^h\,30^m$ and 7^h, and for parallaxes greater than $57'$, while the terms for other hours are more generally negative.

But it appears to be better to wait for the examination of other observations than to attempt to found a formula on these circumstances.

4. *Correction of the Heights for Lunar Declination.*—Table XX. of Mr. LUBBOCK will supply the means of determining the law of the effect of the lunar declination upon the height, being a Table of the heights arranged according to intervals of 3° of declination, and according to the time of the moon's transit. We shall place here the Table, expressed in hundredths of feet.

TABLE showing the Difference in the Height of High Water, and the Mean Height for every Three Degrees of the Moon's Declination.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	− 10	+ 07	+ 06	+ 02	+ 04	− 06	− 10	− 08	− 34	− 42
0 30	+ 09	− 01	+ 06	− 05	+ 14	+ 10	− 03	− 15	− 10	− 20
1 0	+ 23	− 10	+ 11	− 01	− 02	+ 07	+ 02	+ 03	− 07	− 35
1 30	+ 34	− 13	+ 16	+ 08	− 04	+ 07	+ 12	+ 13	− 06	− 45
2 0	+ 40	− 25	+ 10	+ 15	+ 03	− 01	+ 15	− 03	− 15	− 25
2 30	+ 39	− 25	+ 08	+ 25	+ 22	+ 01	+ 24	− 18	− 18	+ 01
3 0	+ 27	− 10	+ 03	+ 27	+ 21	+ 04	+ 21	− 09	− 28	− 09
3 30	+ 22	+ 13	....	+ 31	+ 16	+ 10	+ 17	+ 01	− 34	− 20
4 0	+ 14	− 01	+ 54	+ 20	....	+ 08	+ 12	+ 03	− 32	− 18
4 30	+ 17	− 05	+ 16	+ 10	− 10	+ 06	+ 14	− 08	− 13	− 08
5 0	+ 21	− 08	+ 37	+ 07	− 01	+ 09	+ 12	− 28	− 12	− 08
5 30	+ 22	− 09	+ 53	+ 04	+ 04	+ 08	+ 04	− 05	− 08	− 21
6 0	+ 13	+ 15	+ 51	+ 18	+ 16	+ 13	+ 03	+ 11	− 19	− 37
6 30	− 09	+ 37	+ 19	+ 27	+ 22	+ 11	− 03	− 06	− 45	− 49
7 0	+ 19	− 02	+ 31	+ 56	+ 26	+ 04	0	− 46	− 24	− 53
7 30	+ 52	+ 38	+ 32	+ 64	+ 15	− 13	− 08	− 35	− 19	− 70
8 0	+ 48	+ 60	+ 44	+ 57	+ 47	+ 17	− 07	− 28	− 20	− 61
8 30	+ 22	+ 71	+ 43	+ 25	+ 74	+ 36	− 10	− 28	− 37	− 64
9 0	+ 43	+ 65	+ 32	+ 15	+ 58	+ 25	− 10	− 28	− 50	− 41
9 30	+ 66	+ 54	+ 17	+ 28	+ 37	+ 06	− 12	− 31	− 56	− 43
10 0	+ 58	+ 58	+ 37	+ 44	+ 36	+ 06	− 06	− 26	− 37	− 44
10 30	+ 32	+ 54	+ 44	+ 42	+ 21	+ 02	− 15	− 29	− 19	− 64
11 0	+ 15	+ 41	+ 31	+ 31	+ 13	+ 05	− 15	− 14	− 30	− 61
11 30	+ 12	+ 34	+ 26	+ 28	+ 12	− 08	+ 02	+ 18	− 38	− 44
Sums {	+648	+547	+627	+584	+461	+195	+138	+ 49	0	+ 1
	− 19	−109	0	− 6	− 17	− 28	− 99	−365	−611	−882
	+629	+438	+627	+578	+444	+167	+ 39	−316	−611	−811

It is clear that these sums decrease faster for the large declinations than for the small ones, and the series is tolerably regular with the expression of the number corresponding to declination 3°, which appears to be affected by some anomaly. If we reject this term, and subtract 629 from each of the terms, we find for

Declination	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction	0	(−191)	−2	−51	−185	−462	−590	−945	−1240	−1440

It will appear that the law may be expressed nearly by $-7300 \sin^3 \delta$, which gives

−0 −20 −79 −178 −315 −490 −697 −937 −1207 −1504.

This agrees pretty well, except for the smaller numbers, which are obviously irregular. Hence, if Δ be the mean declination, we shall have the correction to be applied to the mean sums $= 7300 (\sin^3 \Delta - \sin^3 \delta)$; and the correction to the single terms will be $304 (\sin^3 \Delta - \sin^3 \delta)$.

If we suppose the mean declination to be 16° 45′, as appeared in the correction for the times, $7300 \sin^3 \Delta = 608$, and the corrections are,

Declination ..	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction ..	608	588	529	430	293	118	− 89	−329	−599	−896

Hence

	25	24	22	17	11	4	−4	−14	−25	−38

are the corrections for the single terms of the Table.

We shall now apply this correction with a negative sign, in order to consider the law of the residual phenomenon.

TABLE XX. freed from the Term 304 ($\sin^2 \Delta - \sin^2 \delta$).

Decl.	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Corr.	25	24	22	17	11	4	−4	−14	−25	−38
h m										
0 0	−35	−17	−16	−15	− 7	−10	− 6	+ 6	− 9	− 4
0 0	−15	−25	−16	−22	+ 3	+ 6	+ 1	− 1	+15	+18
1 0	− 2	−35	−11	−18	−13	+ 3	+ 6	+17	+18	+ 3
1 30	+ 9	−37	− 6	− 9	−15	+ 3	+16	+27	+19	− 7
2 0	+15	−49	−12	− 2	− 8	− 5	+19	+11	+10	+13
2 30	+14	−49	−14	+ 8	+11	+ 3	+28	− 4	+ 7	+39
3 0	+ 2	−34	−18	+10	+10	0	+25	+ 5	− 3	+29
3 30	− 3	−11	....	+04	+ 5	+ 6	+21	+15	− 9	+18
4 0	−11	−25	+32	+ 3	....	+ 4	+16	+17	− 8	+20
4 30	− 8	−29	−06	− 7	−21	+ 2	+18	+ 6	+12	+30
5 0	− 4	−32	+15	−10	−12	+ 5	+16	−14	+13	+30
5 30	− 3	−33	+31	−13	− 7	+ 4	+ 8	+ 9	+17	+17
16 0	−12	−19	+29	− 1	+ 5	+ 9	+ 7	+25	+ 6	+ 1
6 30	−34	+13	− 3	+10	+11	+ 7	+ 1	+ 8	−20	−11
7 0	− 5	−26	+ 9	+39	+15	0	+ 4	−32	+ 1	−15
7 30	+27	+14	+10	+47	+ 4	−11	− 4	−21	+ 6	−32
8 0	+23	+36	+22	+40	+36	+13	− 3	−14	+ 5	−23
8 30	− 3	+47	+21	+ 8	+63	+32	− 7	−14	−12	−26
9 0	+18	+41	+10	− 2	+47	+21	− 7	−14	−25	− 3
9 30	+41	+30	− 5	+11	+36	+ 2	− 8	−17	−31	− 5
10 0	+33	+34	+15	+27	+25	+ 2	− 2	−12	−12	− 6
10 30	+ 7	+30	+22	+25	+10	− 2	−11	−15	+ 6	−26
11 0	−10	+17	+ 9	+ 4	+ 2	+ 1	−11	0	− 5	−23
11 30	−13	+10	+ 4	+11	+ 1	−12	+ 6	+32	−13	− 6

Though this Table exhibits great anomalies, it appears clear that all the heavy minus terms, and only small positive terms, are in the upper left-hand and lower right-hand quarter; and that all the heavy positive terms, and only small negative terms, occur in the upper right-hand and lower left-hand quarters. Also the terms are on the whole larger in the outer than in the inner columns. It appears probable, therefore, that the law from which this proceeds involves a term $d\ (\sin^2 \delta - \sin^2 \Delta) \sin 2\varphi$, which would give such a result; but the coefficient of this term cannot be determined satisfactorily; and hence the effect of declination in the moon is probably of the form

$$(\sin^2 \Delta - \sin^2 \delta)\ (7300 - d \sin 2\varphi).$$

Hence the correction to be applied to the height calculated for declination 0, is

$$- \sin^2 \delta\ (7300 - d \sin 2\varphi),$$

or

$$- \sin^2 \delta \left(7300 + d \cos 2\ (\varphi + 45^\circ)\right).$$

We may make a remark with respect to Mr. Lubbock's Tables for the heights, similar to one we made with respect to those for the times. Table XVI., which gives the differences of height for each hour of the moon's transit in the different calendar months, is in reality composed mainly of the effects of the moon's declination. In order to obtain these effects from the Table, we should have to eliminate the effects of the sun (including the effects of the equation of time). By this means we should obtain a result agreeing in part with that which we have obtained from Table XX.; but the accuracy of this result would necessarily be less than of that already obtained, and I shall omit it.

5. *The Solar Correction of the Heights.*—If we take the means of each month in Table XVI., we have the sun's effect on the heights in that month. These are as follow:

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
−7	−23	−8	0	+17	+8	+8	−9	−1	+9	+8	+4

This, like the solar correction for the time, passes from positive to negative, and from negative to positive, four times in the course of the year, but has its maxima and minima of unequal magnitude, and at unequal intervals. Hence we may, as in the case of the times, express it by $m \sin 2\,(\theta - \mu) + n \sin\,(\theta - \nu)$, where θ is the sun's longitude; and we may account for this form by considering that the former term is the effect of declination, and the latter term the effect of parallax. To this is to be added the effect of the equation of time, in order to obtain the whole of the solar correction.

Recapitulation.—Hence it appears that the result of the London Dock observations, which we have now examined, may be expressed in the following manner.

If λ' be the corrected establishment, S' the semimenstrual inequality of the time of high water, P' the correction for lunar parallax, Q' the correction for lunar declination, Q the solar correction, and if φ be the mean time of the moon's transit, we have for the time of high water

$$\varphi + \lambda' + S' + P' + Q' + Q.$$

In this expression it has appeared that

$$\tan 2\,S' = \frac{h \sin 2\,(\varphi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)}; \quad \frac{h'}{h} = 2{\cdot}9887; \quad \alpha = 2 \text{ hours.}$$

$$P' = (P - p)\,\{B + B \sin 2\,(\varphi - \beta)\}; \quad B = 3^m; \quad \beta = 1 \text{ hour.}$$

$$Q' = (\sin^2 \Delta - \sin^2 \delta)\,\{C + D \sin 2\,(\varphi - \gamma)\};$$

$$\Delta = 16^\circ\,45', \; C = 132^m, \; D = 84^m, \; \gamma = 4 \text{ hours.}$$

$$Q = m \sin 2\,(\varphi - \mu) + n \sin\,(\varphi - \nu),$$

m, n being small, and their determination here omitted.

In like manner, if l be the height of the mean high water, s' the semimenstrual change, p' the correction due to lunar parallax, q' the correction due to lunar declination, q the solar correction, the height of high water is

$$l + s' + p' + q' + q.$$

In this expression, the numbers being feet, l at the London Docks is 21·33 above the origin of the measures used in the Tables.

$$s' = 1{\cdot}7 \cos(2\varphi - 51^\circ) - {\cdot}23 \sin(4\varphi - 30^\circ)$$
$$p' = (p - \mathrm{P})\{{\cdot}17 + b \cos 2\varphi\}$$
$$q' = (\sin^2 \Delta - \sin^2 \delta)\{73 - d \cos 2(\varphi - 45)\}$$
$$q = m \sin 2(\varphi - \mu) + n \sin(\varphi - \nu),$$

b, d being not clearly shown by the London observations to be constant terms; and m, n, as before, being small, and their determination for the present omitted.

CHAP. III. *Comparison of the preceding Results with the Theory.*

I shall now compare the preceding results with the theory of DANIEL BERNOULLI, according to which the waters of the ocean assume nearly the form in which they would be in equilibrium under the action of the sun and moon; and a supposition being made that the pole of the fluid spheroid follows the pole of the spheroid of equilibrium at a certain distance (namely, at an hour-angle λ'), and that the equilibrium corresponds to the configuration of the sun and moon, not at the moment of the tide, but at a previous moment, at which the right ascension of the moon was less by a quantity α.

I take this theory rather than that of LAPLACE, not only because of the difficulty and labour of the comparison in the latter case, but also because the hypothesis on which LAPLACE's solution proceeds appears to me likely to affect the results, so as to make them differ altogether from those of the real case; and because the assumption, by means of which his solution is obtained, appears to me to be very insecure.

According to the theory of BERNOULLI, we have

$$\tan 2(\theta - \lambda') = \frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)}; \quad \ldots\ldots\ldots\ldots \quad (1.)$$

where θ' is the hour-angle corresponding to the place of high water measured from the moon, φ the hour-angle of the moon from the sun, h, h' the heights of the solar and lunar tides, λ' the hour-angle by which the tide follows the pole of equilibrium, α the *retardation*, or difference of right ascension of the moon due to the age of the tide.

Neglecting the effects of parallax and declination, this expression gives the law of the semimenstrual inequality; and this, as we have already said, agrees very clearly with observation, assuming proper values of $\frac{h}{h'}$, and of α.

But we find here some circumstances in which the theory and observation are difficult to reconcile. The value of $\frac{h'}{h}$, the ratio of the lunar and solar tide, ought to be the same at all places. We find, however, that the Brest observations give 2·6167, while the London observations require it to be 2·9887; and other places give values still more different.

Also the differences of the value of α for different places might be supposed to

depend necessarily upon the time of the transmission of the tide from one place to another, and therefore to increase as we follow the tide. But it appears that though at Portsmouth this retardation is intermediate between that at Brest and at London, as it might be expected to be from the course of the tide-hours, yet that at Plymouth, where the tide is five or six hours earlier than it is at Portsmouth, the retardation implies a tide as late as London.

Leaving, however, these anomalies to be removed or confirmed by the accumulation and discussion of observations, I proceed to the effects of parallax and declination.

1. If from any cause h' receive a small increment $\delta h'$, we can easily find the corresponding change, $\delta \tan 2(\theta' - \lambda')$, in the first side of equation (1.). We have

$$\delta \tan 2(\theta' - \lambda') = \frac{h \sin 2(\varphi - \alpha) \cdot \delta h'}{(h' + h \cos 2(\varphi - \alpha))^2}.$$

Let h' represent the mean value, and $\delta h'$ any deviation from the mean; and let S' represent the semimenstrual inequality, that is, the value of $\theta' - \lambda'$ freed from effects of declination and parallax. Then

$$\tan 2S' = \frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)}; \text{ whence}$$

$$\delta \tan 2(\theta' - \lambda') = -\frac{\tan^2 2S'}{\sin 2(\varphi - \alpha)} \cdot \frac{\delta h'}{h} \quad \ldots \ldots \ldots \ldots \quad (2.)$$

Now, *cæteris paribus*, h' is as the cube of the parallax. If, therefore, P be the mean parallax, and p any other,

$$h' + \delta h' = h' \frac{p^3}{P^3} = h' \left(1 + \frac{p - P}{P}\right)^3 = h' + 3h' \frac{p - P}{P},$$

omitting other terms, because $p - P$ is small.

Hence $\delta h' = 3h' \frac{(p - P)}{P}$; and when we make this substitution, equation (2.) gives the change in the first side due to the effect of lunar parallax.

Since the arc $\theta' - \lambda'$ is small, we may put it for its tangent; hence, making the above substitution and calling the effect of lunar parallax P′,

$$2P' = -\frac{h'}{h} \frac{\tan^2 2S'}{\sin 2(\varphi - \alpha)} \cdot 3 \frac{(p - P)}{P}.$$

As a first approximation to the general form of the result, we may put $\frac{h}{h'} \sin 2(\varphi - \gamma)$ for $\tan 2S'$, since $\frac{h}{h'}$ is a fraction (about one third), and since the general course of the two functions, $\sin 2(\varphi - \alpha)$ and $\tan 2S'$, agrees.

Hence we should have

$$P' = -\frac{3h}{h'} \sin 2(\varphi - \alpha) \cdot \frac{p - P}{P}$$

$$P' = (P - p) \cdot B \sin 2(\varphi - \alpha);$$

B being a constant quantity.

The expression which we obtained from observation was

$$P' = (P - p)\left(B + B \sin 2(\phi - \beta)\right),$$

of which the second term agrees in form with the one given by the theory, except that the angle β is different from α; but the first term should not occur according to the theory as hitherto stated.

2. Again, for the effects of lunar declination on the time of high water.

If k' be the value of h' when the place of observation and the pole of the lunar tide spheroid are both in the equator, and if ε be the difference of declination of the place and the pole in any other situation, we shall have $h' = k' \cos^2 \varepsilon$ nearly.

In the course of a tide-day there are two tides, corresponding to two positions of the tidal spheroid; and if l be the latitude of the place, δ the declination of the moon, the two corresponding differences of declination will be $l - \delta$ and $l + \delta$, the pole of the spheroid being supposed to have the same declination as the moon has at the moment of the origin of the tide (that is, when the moon's right ascension was less by α than it is at the moment of the tide).

Then, in the first case,

$$\begin{aligned} h' &= k' \cos^2 \varepsilon = k' \cos^2 (l - \delta) = k' (\cos l \cos \delta - \sin l \sin \delta)^2 \\ &= k' \{\cos^2 l - 2 \sin l \cos l \sin \delta \cos \delta - (\cos^2 l - \sin^2 l) \sin^2 \delta\} \\ &= k' \cos^2 l - \tfrac{1}{2} k' \sin 2l \sin 2\delta - k' \cos 2l \sin^2 \delta. \end{aligned}$$

In the second case, similarly,

$$h' = k' \cos^2 l + \tfrac{1}{2} k' \sin 2l \sin 2\delta - k' \cos 2l \sin^2 \delta.$$

In order to find the effect of the declination upon each tide, we should put for $\delta h'$ the quantities $-\frac{1}{2} k' \sin 2l \sin 2\delta - k' \cos 2l \sin^2 \delta$, and $+\frac{1}{2} k' \sin 2l \sin 2\delta - k' \cos 2l \sin^2 \delta$ respectively.

Thus, according to the theory, the effect of declination on the two tides of the same day should be different. This difference is very much modified by the circumstances in which the actual state of the ocean differs from the theoretical state: the difference of the diurnal tides may, however, be detected in the observations at most places of the earth's surface, perhaps at almost all. But there are peculiar circumstances in the port of London which affect this difference, and obliterate it: the tide at London is composed of two tides, which differ by half a day from each other, and hence the difference of the two semidiurnal tides disappears altogether. Therefore, instead of the effects of declination on the two semidiurnal tides, we must take the mean of these effects, which is $-k' \cos 2l \sin^2 \delta$.

Hence if Q' represent the effect of lunar declination on the time of high water, we have by equation (2.) (substituting $-k' \cos 2\theta \sin^2 \delta$ for δh, and putting the arc for $\tan 2(\theta' - \lambda')$),

$$2Q' = \frac{\tan^2 2S'}{\sin 2(\phi - \alpha)} \frac{k'}{h} \cos 2l \sin^2 \delta.$$

In this expression we have $k' \cos^2 l$ for h' in the value of $\tan S'$; but it is clear that

in considering the effect of solar declination, we should in like manner have $k \cos^2 l$ for h, whence the value of $\tan 2\,S'$ in equation (1.) would remain unaltered.

Putting, as before, $\frac{h}{p} \sin 2\,(\varphi - \alpha)$ for $\tan 2\,S'$, the equation becomes

$$2\,Q' = \frac{h'}{h} \cos l \sin 2\,(\varphi - \alpha) \,.\, \sin^2 \delta\,; \text{ or}$$

$$Q' = D \sin 2\,(\varphi - \alpha) \,.\, \sin^2 \delta,$$

where D is a constant quantity.

In this expression the correction for declination is supposed to be applied to the time of high water, calculated for the moon and the place, both in the equator. But in our tables this correction is applied to the mean place. Let Δ be the value of the declination at this mean place; then the correction for that case is $D \sin 2\,(\varphi - \gamma) \sin^2 \Delta$, and therefore,

$$Q' = (\sin^2 \delta - \sin^2 \Delta)\, D \sin 2\,(\varphi - \alpha)$$

is the correction to be applied to the mean.

The correction according to observation was

$$Q' = (\sin^2 \delta - \sin^2 \Delta)\,\big(C + D \sin 2\,(\varphi - \gamma)\big).$$

The second term agrees with the theory, except that the arc γ is different from α: the first term, $C\,(\sin^2 \delta - \sin^2 \Delta)$, has nothing corresponding to it in the theory.

3. We now proceed to the theoretical laws which regulate the height of high water.

If θ, θ' be the distance of any place in the equator from the places to which the sun and moon are vertical (these luminaries being supposed to be in the equinoctial), the height of the water at the place will be $\frac{1}{2}\big(h \cos 2\,(\theta - \lambda) + h' \cos 2\,(\theta' - \lambda')\big)$ above the mean level; and if θ' be taken the distance of the highest water from the moon, then

$$h \cos (2\,\theta - \lambda) + h' \cos 2\,(\theta' - \lambda')$$

will be the whole tide, which call y.

Now we have

$$\tan 2\,(\theta' - \lambda') = \frac{h \sin 2\,(\varphi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)},$$

where $\theta + \theta' = \varphi$.

Hence we find

$$\cos 2\,(\theta' - \lambda') = \frac{h' + h \cos 2\,(\varphi - \alpha)}{\sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}}}$$

$$\cos 2\,(\theta - \lambda) = \frac{h + h' \cos 2\,(\varphi - \alpha)}{\sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}}}$$

$$y = \sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

If, as before, δy represent the variation of y in virtue of any variation of h',

$$\delta y = \frac{h' + h \cos 2\,(\varphi - \alpha)}{y} \,.\, \delta h'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

The semimenstrual inequality of the heights is given by equation (3.).

Expanding, we have

$$y = \sqrt{(h'^2 + h^2)}\left\{1 + \frac{h\,h'}{(h'^2 + h^2)}\cos 2\,(\varphi - \alpha) - \frac{h^2 h'^2}{8\,(h'^2 + h^2)^2}\cos^2 2\,(\varphi - \alpha) +, \&c.\right\}$$

$$= \sqrt{(h'^2 + h^2)} - \frac{h^2 h'^2}{16\,(h'^2 + h^2)^{\frac{3}{2}}} + \frac{h\,h'}{\sqrt{(h'^2 + h^2)}}\cos 2\,(\varphi - 2\,\alpha) - \frac{h^2 h'^2}{16\,(h'^2 + h^2)^{\frac{3}{2}}}\cos(4\,\varphi - 4\,\alpha);$$

omitting ulterior terms, since the coefficients diminish according to powers of $\dfrac{\frac{h}{h'}}{1 + \left(\frac{h}{h'}\right)^2}$.

Hence the variable part of this expression is of the form

$$\mathrm{K}\cos(2\,\varphi - 2\,\alpha) - \mathrm{L}\cos(4\,\varphi - 4\,\alpha).$$

The expression of the semimenstrual inequality of heights, found from observation, was (in feet)

$$\mathrm{S}' = 1{\cdot}7\cos(2\,\varphi - 51^\circ) - {\cdot}23\cos(4\,\varphi - 30^\circ),$$

which agrees with the theoretical expression, except as to the values of the arcs which take the place of $2\,\alpha$ and $4\,\alpha$.

4. To find the effect of lunar parallax on the heights, substitute as before for $\delta\,h'$, in equation (3.), and let p' be this effect; then

$$p' = \frac{h' + h\cos 2\,(\varphi - \alpha)}{y}\,.\,3\,h'\frac{p - \mathrm{P}}{\mathrm{P}}.$$

Here y is the mean height.

Therefore p' is of the form $(p - \mathrm{P})\left(a + b\cos 2\,(\varphi - \alpha)\right)$.

The form as given by observation is $(p - \mathrm{P})\,(a + b\cos 2\,\varphi)$, where, however, the existence and constancy of b are doubtful.

5. To find the effect of lunar declination on the heights, substitute for $\delta\,h$, as before, $-\,k'\cos 2\,l\sin^2\delta$. We thus find from equation (3.), q' being the effect,

$$q' = -\,\frac{h' + h\cos 2\,(\varphi - \alpha)}{y}\,k'\cos 2\,l\sin^2\delta\,;$$

and referring the correction to the mean declination Δ, it becomes of the form

$$q' = (\sin^2\Delta - \sin^2\delta)\left(c + d\cos 2\,(\varphi - \alpha)\right).$$

The form given by observation was

$$q' = (\sin^2\Delta - \sin^2\delta)\left(c + d\cos 2\,(\varphi + 45^\circ)\right),$$

where, however, d was not determined as to quantity, the observations being too anomalous.

It appears, therefore, that the results of observation and theory for the variations of height agree as to form, with the exception of the epochs α, β, γ.

Chap. IV. *Reflexions on the Theory.*

It would be unsafe to attempt to deduce any general views concerning the laws of the tides from the preceding investigations. It is very unlikely that the discussion of observations at any one place, and those the very first set which have been systematically discussed, should exhibit clearly the true principles of the theory: and besides this, it so happens, that the phenomena of the tides at London are in some measure masked by a curious combination of circumstances, namely, by the mouth of its river being on the side of an island, turned away from the side on which the tide comes, and so situated that the path of the tide round one end of the island is just twelve hours longer than round the other. It will require the accumulation and discussion of many large masses of observations, at various places, to put us in firm possession of the laws of the phenomena as given by experience; and this road, whether or not it be the *only* practicable way of arriving at the true theory, is at least that to which, founding our expectations on the past history of science, we may look with most hope. When we consider the enormous accumulation of observed phenomena and empirical laws which preceded the discovery of the true principles of the heavenly motions, we may easily suppose that we are only at the outset of what we have to do, in order to obtain the same success with regard to the tides: and we may, from the same consideration, find additional motives to desire that such observations may be made, and such existing observations may be discussed, as may most speedily lead us to a complete and scientific knowledge of the subject.

But though we cannot make our inferences from the preceding investigation with confidence, there are some reflexions concerning the mode in which the forces of the sun and moon manifest themselves in the tides, which are suggested by the comparison made in the foregoing pages, and which I will venture to state. The confirmation or refutation of these views must depend on future investigations of the same nature as that contained in this memoir: in the mean time, the views seem fitted to give some additional impulse to the curiosity with which all men of science must now look upon the progress of this subject.

Among the inequalities considered in this memoir, those in which the empirical laws are the clearest and the anomalies the smallest (after the semimenstrual inequalities,) are the inequalities of the time of high water, depending on the moon's parallax and declination. In these the comparison of the law, from theory and from observation, may be stated as follows:

Observation.	Theory.
$P' = (P - p)\left(B + B \sin 2(\varphi - \beta)\right);$	$(P - p)\, B \sin 2(\varphi - \alpha).$
$Q' = (\sin^2\delta - \sin^2\Delta)\left(C + D \sin 2(\varphi - \gamma)\right);$	$(\sin^2\delta - \sin^2\Delta)\, D \sin 2(\varphi - \alpha).$

It will be observed, that in each of these cases observation gives, in P' and Q', a term depending on the parallax and on the declination, (namely, the terms $(P - p)\, B$

and $(\sin^2\delta - \sin^2\Delta)\,C$,) which term is not given by the theory; besides giving another term which coincides with the one given by the theory. The latter term depends on the hour of the moon's transit, and vanishes twice in the course of a semilunation; the former term in each case is independent of the time of the moon's transit, and depends only on the parallax and on the declination.

Now P′ and Q′ are the corrections to which $\theta' - \lambda'$ is subject, where θ' is the hour-angle of the tide from the moon. In the theory, λ' is supposed to be constant, so that the variation of $\theta' - \lambda'$ alone affects θ'.

But since $\theta' = \lambda' + (\theta' - \lambda')$, if λ' were affected by an inequality arising from parallax equal to $(P - p)\,A$, we should have, taking the theoretical value of the variation of $\theta' - \lambda'$ due to this cause, and adding it to the value resulting from the common theory, the whole variation of $\theta' = (P - p)\left(A + B \sin 2(\varphi - \gamma)\right)$.

In like manner, if λ' were affected by an inequality equal to $(\sin^2\delta - \sin^2\Delta)\,C$, and $\theta' - \lambda'$ by the inequality resulting from the theory, we should have for the whole inequality in θ' arising from declination, $(\sin^2\delta - \sin^2\Delta)\{C + D \sin 2(\varphi - \gamma)\}$.

Now these expressions agree with those which we have obtained from observation, excepting that we have other arcs in the place of the arc α. It appears, therefore, that the empirical laws will be verified by supposing λ' to be affected by inequalities depending upon the parallax and declination of the moon, but having an epoch different from that of the semimenstrual inequality.

The quantity λ' is the hour-angle by which the lunar tide follows the high water of the lunar spheroid of equilibrium. It appears, therefore, that the physical statement of the result just obtained is this, that the distance at which the actual elevation of the waters follows the position of equilibrium, varies as the parallax and declination of the disturbing luminary vary.

This distance was, in the theory, assumed to be constant; but there is no obvious physical reason why it may not change with changes of the force by which the fluid spheroid of equilibrium is determined. This distance, or *lagging*, of the pole of the watery spheroid behind the place which it would occupy if the earth and luminary were at rest, is owing to the resistance of the shores and of the parts of the water amongst each other; and its amount is determined by the amount of these resistances. But we are very far from being able to trace the mode in which these causes operate, so as to be entitled to affirm that changes, and even small changes, in the force or velocity of the disturbing body, may not produce corresponding changes in the extent of this lagging.

In fact, there seems to be good reason to suppose, from other circumstances, that the force and velocity of the disturbing body do affect the distance by which the actual elevation lags behind the elevation of equilibrium. For λ and λ', the lagging in the case of the solar and of the lunar tide, are quite different; the former (for the London Docks) being $3^h\,25^m$, the latter $1^h\,25^m$ *. It is true that this difference of 2^h is, in the

* See Mr. Lubbock's Memoir, Philosophical Transactions, 1831, p. 387.

theory, got rid of by supposing the actual tide to be referred to a configuration of the sun and moon anterior by $2\frac{1}{2}$ days to the configuration at the time of the tide. But since we refer the effects of parallax and declination to the parallax and declination contemporaneous with the tide, we must look for an analogy only when we do the same in the other case.

In the semimenstrual inequality, as determined from observation, there is no such discrepancy with theory as compels us to suppose a change in λ'. But this forms no objection to our view; for if, in the course of a semirevolution of the moon, there were a periodical change in λ', this must have the same cycle as the change in $\theta' - \lambda'$, and would therefore be confounded with that change, and would not result in a separate *form* from our discussion.

But, moreover, the difference of the *quantity* of the semimenstrual inequality at different places, which we have already shown to exist, supplies a confirmation of the opinion here put forwards. For this difference implies that the tide travels from one given place to another in different times at different periods of a semilunation; that is, it implies that the velocity of the tide-wave is different in different configurations of the sun and moon, that is, under different circumstances of the tide-producing forces. And this agrees with our doctrine, that the amount of lagging is different under different circumstances of those forces; for if the amount of lagging of the tide elevation go through a cycle of changes in a certain period, the velocity with which this elevation travels will also go through a cycle of changes in the same period. And this difference of the semimenstrual inequality at different places, does appear to betray a semimenstrual inequality affecting λ', the amount of the inequality varying with the place; and this variation, added to the theoretical semimenstrual inequality which affects $\theta' - \lambda'$, and which is the same for all places, makes up the empirical semimenstrual inequality of θ', given by our mode of investigation, which thus appears to be different at different places.

Taking all these reflexions into consideration, there appears to be good reason to believe that the amount of the lagging of the tide behind the equilibrium-tide is really affected by changes in the distances and velocities of the disturbing luminaries.

There is another circumstance in which the empirical differ from the theoretical laws: the epochs β, γ of the changes due to parallax and declination are different from the epoch α of the semimenstrual inequality.

The physical statement of this result is, that the time required to transmit to any port the general effect of the tide-producing forces, and the time required to transmit to the same port the effects of particular changes in these forces, are different. And of this result we may say, in the same manner as of the former, that we see far too obscurely the causes which determine the amount of this interval in one case, to assert that it must necessarily be the same under different circumstances. But we may illustrate this subject somewhat further. We may suppose an imaginary mean moon, moving uniformly in the equator, at a constant distance from the earth, to produce the mean tide; another auxiliary moon, by moving directly to or from the earth, in

the line of the mean moon, to produce the inequality which arises from parallax; another auxiliary moon, by moving north and south in the meridian of the moon to produce the inequality which arises from declination. Now the tides produced by all these moons will require some time for the operation of the forces to take effect; that is, they will correspond to positions of the moon at a time anterior to the actual time. But there seems not to be the smallest reason to conclude that these anterior times will all be anterior by the same interval: the contrary, rather, is obvious. It is clear, for instance, that a tide oscillating in a north and south direction in the Atlantic and Pacific Oceans, will take a different portion of time to obey the forces which produce it, from the general tide which travels from east to west round the earth in virtue of the diurnal motion, and impinges against the broad sides of the great continents. We may therefore expect to find the epochs of all these partial tides different; and as every separate term in the expression of an inequality may be considered as representing a different tide, there will be nothing inconsistent with the best physical notions we can yet form on the subject in finding the epochs of the arguments of every separate term of our formulæ different from one another.

It appears, then, that though the equilibrium theory, taken in combination with the preceding considerations, may very probably give us the general form of the terms, and the variable part of the arcs on which they depend, the constant epoch which occurs in each of these arcs, and which determines when the inequality vanishes and reaches its maximum, will probably have to be determined in all cases by observation.

I will observe further, that not only the epochs, but the coefficients of each of these terms will probably have to be determined for the most part from observation. For the tides, though in the theory to which we refer considered as representing positions of equilibrium of a fluid, are in fact the results of its motion; and it is not at all clear that the elevation which results from the motion will be equal to the elevation which would be requisite for equilibrium. It is true that there must be always a tendency to this equilibrium-elevation so long as the actual elevation is greater or less than that; but this tendency may never fully appear in the circumstance of the tide; since the tide-producing forces have to supply also a residue of force which must be employed in producing the motion of the fluid.

Moreover, the motion of the fluid is of the nature of an oscillation, so that series of increasing and diminishing oscillations at intervals of a half-day, a day, and other intervals, pass through any given part of the ocean. Now it is physically, not only possible but certain, that each oscillation in each series is affected by those which precede it in the same series, and affects those which succeed it, so that their relative magnitude is different from what it would otherwise be. And the effects thus produced will depend upon the depth of the ocean, the form of its shores, and other causes, of which it is impossible to estimate the result *à priori*.

Even in the case of the semimenstrual inequality, which in its form agrees so

closely with the theory, and which in its amount appears to depend only on the ratio of the forces of the sun and moon, we find that in fact its amount is different at different places, as we have already stated. We cannot expect, therefore, that the amount of the corrections for parallax and declination will agree very exactly with those from theory; and till the empirical corrections are more certainly and generally determined, I have not thought it worth while to make the comparison.

But though there is at present this uncertainty respecting the amount of the inequalities of the tide, I do not conceive that there can be any doubt that the forms of these corrections are such as I have stated them. In the case of the times of high water especially, the general course of the variations of the quantities is as regular as can be expected, and as is requisite for the establishment of our formulæ. The heights are much more anomalous; probably they are more affected by winds, &c., than the times are: and when we reflect that the tide at London may be affected by the operation of causes in a remote part of the ocean, propagating their effect by the progression of the tide-wave, we shall not be surprised at considerable deviations from rule. The trade winds and other winds of the tropical regions may be felt in our tides, and may even affect the means of long series of observations; for it is to be recollected that the averages which we obtain are not the averages of the effects of the sun and moon alone, but the averages of their effect, together with that of meteorological causes; and it is very conceivable that the latter average may not vanish in the long run. It is moreover to be observed, that the peculiar circumstances of London, in having a tide compounded of two tides, arriving by different roads after journeys of different lengths, may easily be supposed to give rise to additional chances of irregularity.

It may not be superfluous to remark, that, independently of such a combination of circumstances, there is nothing in the situation of the port of London to diminish the value of tide observations there. The length and windings of the river by which the tide reaches the port present no objection to the comparison of the observations with theory. These circumstances may modify the tide, but they modify it alike every day, or at least alike at like periods of the tidal cycles, and therefore they introduce no irregularity. Indeed, there are some reasons for believing that the tides in rivers and deep sounds are more regular than those on the open coast; and at any rate, as they are generally larger in such situations, their variations are more observable.

Concluding Observations.

It appears from the preceding investigations and considerations, that the following are now the most important steps from which any great improvement of our knowledge on this subject may be hoped.

Large collections of observations at other places must be discussed in a manner resembling that employed for the London observations by Mr. Dessiou. The Brest and the Liverpool observations would be excellent materials for such operations. We

must thus ascertain whether the empirical forms of the corrections for parallax and declination, deduced from these, agree with those obtained for London in the preceding pages. If they do, the coefficients must be compared with each other and with the theory, in order to determine the most promising mode of pursuing the latter.

The empirical formulæ obtained in the preceding pages represent the observations with tolerable exactness; probably they agree with them almost as well as any formulæ would do, and as well as the observations agree with each other. These formulæ might be used in calculating tide tables as readily as any other empirical rules; and the tables so calculated might be compared with observations made at London. Such a comparison, continued long enough, would disclose any additional corrections which may be requisite in this mode of calculating tide tables.

Tide observations are now made at the Katharine Docks, with good apparatus and a judicious system; and, so far as I can judge, with proper care. These will hereafter form materials for a better discussion of the London tides than the London Dock observations, made in a ruder manner, could allow.

III. *On the Position of the North Magnetic Pole. By Commander* James Clark Ross, *R.N. F.R.S. F.R.A.S. F.L.S. &c.*

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THE determination of the position of the Magnetic Poles of the earth has ever been considered a desideratum in the science of magnetism, of the highest importance; and the observations and experiments of the most ingenious and learned philosophers have universally been applied to the solution of this difficult and perplexing problem. Vague and unsatisfactory, however, were the results of the researches and calculations of the most indefatigable and zealous promoters of that science, arising, doubtless, in a great measure, from the discordant observations upon which they were founded,—a discordance which was considered to arise chiefly from the unequal distribution of the magnetic substances contained in the earth, and also from the great distances at which the observations were made from the centres of the powers of those magnetic substances, or, in other words, from the magnetic foci, or poles, of the earth.

The primary cause of magnetic phenomena has always been, and still is, one of the secrets of nature, although several of the laws of magnetism have of late years been gradually developed: and during our absence from England, a greater step perhaps than any former one has been made, through the indefatigable research of Dr. Faraday, by his splendid and convincing proofs of its complete identity with electricity. Still much remains to be accomplished relative to terrestrial magnetism; and accurate observations with good instruments, as near the magnetic poles as possible, and in various directions from them, were long considered amongst the desiderata for completing the magnetic theory of the globe.

These wants, as far as relates to the northern magnetic regions, have been supplied by the expeditions by land and sea that have been sent from England for the discovery of a north-west passage, to traverse the shores of the American continent, and to contribute to the advancement of science in general. In the department of magnetism, in particular, the numerous and accurate observations by their distinguished commanders, and those who accompanied them, have been eminently important. Those made to the north-west of the magnetic pole by Captain Sabine, to the south-west by Captain Franklin, and to the south-east and north-east by Captain Parry, Mr. Fisher, and Captain Forster, have furnished materials that have enabled the British philosophers to point, with a wonderful degree of precision, to the seat of magnetic concentricity.

In contemplating the equipment of the late expedition, a still nearer approach than had yet been attained to that mysterious spot was anticipated from the route that we purposed to pursue; but the smallness of the vessel in which we embarked necessarily limited the number and magnitude of our magnetic instruments. A small dipping-needle by Jones, belonging to the Admiralty, was, together with a number of other instruments, liberally offered for our use; and having been made with much care by that celebrated artist for the use of the party that travelled towards the north pole under Captain Parry, and been found on that occasion to answer every purpose for which it was intended, we did not hesitate to consider it sufficiently large and accurate for this service. A description of the instrument accompanies the Table of Observations made by Captain Parry and Lieutenant Forster in the Appendix to the Narrative of that Voyage (p. 168), and renders any further remarks here unnecessary. It is, however, to be regretted, that prior to our departure from England we had no opportunity of making any observations with that instrument; and a defect in the vertical circle, which was not detected till the spring of the year 1831, has rendered it necessary to reject all the observations on the intensity of the magnetic force made previous to that period.

The annexed Table contains most of the observations that were obtained on the dip of the magnetic needle during our late voyage in the Victory, and seems to require but little explanation. I have considered it proper to record the mean of the readings of each end of the needle in each of its eight positions, because, in looking over the Table, it will be seen that scarcely any two results show any very near accordance, and, in some instances, their differences amount to several degrees. Whether this arises from any imperfection in the instrument, from the method of magnetizing it, or from a variation in the direction of the poles of the needle, I am unable to determine. As the several readings presented themselves, so they were registered; and the resultant dips, although in some instances they show a very considerable difference, yet, upon the whole, their accordance affords a remarkable instance of the tendency of errors (if such they be) to correct each other. Be that as it may, it is proper that these discordances should be known, in order that their cause may be investigated, and that the observations should not obtain a greater degree of dependence than, on examination, they may be found to deserve. Each of the recorded observations is the mean of six to ten readings of each end of the needle in its several positions, and the method employed in the reversion of its poles is that of Du Hamel.

Only three opportunities occurred of observing the dip as we proceeded to the southward of Fury Point to our first winter quarters. But these, together with the variation, &c., were important assistants in calling our attention to the rapid approach we were making towards the magnetic pole. A series of observations during the winter led us to expect that that point would be found directly to the westward of us; but we were unconscious at that time of the existence of an ocean in that direction, and the calculated distance far exceeded anything we could hope to travel over a country

whose rugged shores seemed to forbid the attempt, and to annihilate every hope of its accomplishment. The discovery of the Western Ocean, however, across a narrow neck of land to the south-west, which occurred early the following spring, gave rise to a small party being sent from the ship, to endeavour to trace the shores of the American continent as far to the south-west or west as possible. On that occasion, owing to the smallness of the party, it was found impracticable to carry more instruments than were actually indispensable for determining the outline of the coast along which we might pass. An azimuth compass, of Captain KATER's construction, was the only magnetic instrument that could be taken, and this was, soon after leaving the ship, destroyed by a fall over a precipice at Cape Isabella, soon after I had determined that its north point was directed to the north-west. Its action was uncertain to eight or ten degrees, owing to the extreme weakness of the directive force of the needle.

Imperfect as this indication was, it seemed to cherish the hope of our being able to obtain some interesting magnetic observations; when having been compelled to pass another winter near the same spot, I proposed to conduct a party, guided by some Esquimaux, across the country to the westward, and to endeavour to approach as near as possible to the source of magnetism. We accordingly commenced our journey in the middle of May 1831: but the unfavourable nature of the season prevented my obtaining any observations that could be of assistance to us until we reached the shores of the Western Ocean on the 28th of the month. Here good observations were made under the most favourable circumstances; and the magnetic dip having now increased to 89° 41′, and the horizontal needle pointing to N. 57° W., led us to expect that, at the distance of about thirty-five miles in that direction, we should attain the object of our wishes. That spot being now well within our reach, I did not hesitate to devote the larger part of the day to repeating those observations, anticipating that, after leaving that spot, little assistance could be expected from the horizontal needle in directing our approach to the magnetic pole. Having gained the calculated position on the 1st of June, without having been able, from the unfavourable state of the weather, during that interval, to obtain any more observations, I availed myself of the snow huts of a recently deserted Esquimaux village as observatories, and encamped the party at a sufficient distance to ensure their being beyond the possibility of producing any influence on the needles, &c.

My attention was first of all directed to ascertain, if possible, the direction of the magnetic meridian. For this purpose I suspended horizontally the needle that was used only for the determination of the intensity of the magnetic force, first by three or four delicate fibres of floss silk. It remained, however, exactly in the position in which it was placed. A single fibre of the floss silk was next tried, and lastly a single fibre of flax. All these failing to demonstrate the smallest amount of horizontal attraction, a second needle was treated in a similar manner, and in all these attempts I was equally unsuccessful. The top of the instrument being so constructed as to admit of a half-circle of torsion, this was next tried; but the needle was moved from

its position in nearly the same amount as the arc described by the point of suspension, showing that the smallest amount of torsion was sufficient to overcome the directive energy of the needle.

The needle was now removed to the dipping apparatus, and the following observations on the intensity of the vertical force of the needle were obtained, upon the supposition, that in whatever direction a given number of vibrations in the same arc were made in the shortest time, that might be assumed as the magnetic meridian. The direction of the needle is given in true bearings.

S. 50° W. and N. 50° E.	S. 80° W. and N. 80° E.	N. 70° W. and N. 70° E.	N. 40° W. and S. 40° E.	N. 10° W. and S. 10° E.	N. 20° W. and S. 20° W.
h m s	h m s	h m s	h m s	h m s	h m s
10 34 20	10 37 28·7	10 40 50·2	10 44 3	10 46 59·5	10 49 47·5
43	52·5	41 13·5	26·5	47 23	50 10·5
35 5·2	38 14·5	36·5	49·2	45·5	33·2
27	36·2	57·5	45 10·5	48 7·5	54·7
48·5	58	42 19	32·5	29	51 16·2
36 10·2	39 18·7	40·7	54	50·7	37·5
50 Vibns in 1 50·2	1 50	1 50·5	1 51	1 51·2	1 50

From these observations it was equally impossible to assign a direction to the magnetic meridian, the slight differences being within the limits of the errors of observation, and the amount of the inclination or dip of the needle in each of these directions being precisely the same. A diminution of force, however, may seem to obtain in the directions of S. 10° E. and S. 40° E.; and a direction at right angles to that, S. 75° W., I assumed as the magnetic meridian in the first two sets of dip. The mean of these was 89° 58′ 15″. The next two sets were taken at an angle of 45° to the right of the former, and their mean result was 89° 59′ 46″; and the two last sets, exactly at right angles to the first set, gave the dip 89° 59′. In these last observations the axis of the intensity needle was put in the stead of its own axis, which accounts for the difference in the readings of the needle in its several positions, as will be seen by the table of dips. The reason for my doing this was to provide against the possibility of the observations being influenced by an injury which the axis of the needle was supposed to have sustained, by the great difference that sometimes occurred in its indications. The result of these observations, however, shows that the injury, if it had met with any, did not materially affect the results; so perfectly do the principles of its construction counteract any slight bend in the axis, or any inequality in the balance of the needle.

To complete the observations on the intensity of the magnetic force, and the various experiments which were made, and which it is unnecessary here to notice, occupied the whole of the time that I could devote to that purpose. And although there is a difference, amounting to several minutes, in the different observations made in the same direction of the needle, yet the resultant mean dip in each of the three directions

in which they were obtained placed us as near to the magnetic pole as, with our limited means, we were able to determine. And although it cannot but be a rough approximation, yet it is hardly possible to be more than a few miles from the exact position. It was, at any rate, quite impossible for us to know, now that the horizontal needle had ceased to act, in what direction to proceed for the purpose of approaching it more nearly; for, in order to determine its exact position, the cooperation of several observers, placed at some distance, in various directions of its position, would be necessary. A series of observations, continued for some months, would afford the most important and interesting data. By such means, not only its actual position, but its diurnal, if not its annual motions, could be determined, and furnish the means of investigating most of the phenomena of magnetism which are now exhibited on our globe; and establishing for future ages a most important point of reference, by which any progressive movement may be ascertained, and ultimately brought within the reach of mathematical determination for any given period.

This is precisely what is still wanting; and now that its position is so nearly known, and that it is placed in a spot easy of access, and affording every facility for carrying such a series of operations into effect, it only remains to be considered whether those who have the power to promote such an undertaking may attach sufficient importance to the subject to direct its being carried into execution. It is certainly every way worthy of our country. The science of magnetism, indeed, is eminently British. There is no other country in the world whose interests are so deeply connected with it as a maritime nation, or whose glory as such is so intimately associated with it, as Great Britain. All the late discoveries and improvements are to be attributed to the perseverance of British science, and the encouragement and assistance of an enlightened and liberal Administration. Nor will the name of Felix Booth, Esq. be omitted in the list of our country's most distinguished patriots, whose munificence and princely spirit have furnished the whole pecuniary means of obtaining the results which are now presented to the Society; and, I may fearlessly venture to add, of enabling a few British seamen to plant the flag of their country upon the Northern Magnetic Pole of the earth.

Observations on the Dip of the Magnetic Needle.

Date.	Time of the day.	Poles of the Needle direct.					Poles of the Needle reversed.					Observed Dip.	Remarks.
		Axis direct.		Axis reversed.		Mean.	Axis direct.		Axis reversed.		Mean.		
		Face East.	Face West.	Face West.	Face East.		Face East.	Face West.	Face West.	Face East.			
1831.		° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	
Feb. 15.	Noon.	78 11·5	99 34·73	78 6·23	99 25·67	88 49·53	79 26·17	98 37·67	79 17·33	98 52·5	89 3·42	88 56·47	
28.	1 P.M.	81 30	98 38·78	79 35·60	98 14·50	89 29·72	72 7·86	103 38·44	72 52	105 43·75	88 35·51	89 2·61	
March 1.	2 P.M.	81 42·8	97 52	81 7	96 49·4	89 22·80	77 16	100 27	77 47·42	98 49·6	88 35·0	88 58·90	
4.	Noon.	81 34·8	96 37·4	81 18	97 25	89 13·8	76 30·4	101 10	78 15·6	99 27	88 51·25	89 2·52	
15.	1 P.M.	81 34·7	96 34·5	81 34·2	96 41·2	89 6·15	75 27·5	102 12·3	75 6·9	102 59·5	88 56·55	89 1·35	Mean observed dip at Sheriff's Bay in lat. 70° 1′ N., and long. 91° 54′ W. Variation 96° 12′·3 W. previous to my journey towards the magnetic pole = 88° 57′·04 N. (1831.)
21.	4 P.M.	75 5·67	103 6·17	74 12·83	101 8·33	88 23·14	81 14·5	97 6	81 56·1	95 35·3	88 57·97	88 40·56	
22.	4 P.M.	86 7·63	91 30·17	87 0·0	90 29	88 47·7	86 13	92 35·7	87 19·83	90 37·83	89 11·59	88 59·15	
23.	3 P.M.	86 18·2	91 11·35	87 9·14	90 47	88 51·42	86 24·17	90 17	87 28	91 22	88 47·29	88 49·36	
24.	2 P.M.	81 56·5	96 18·7	81 49·7	96 0·20	89 1·27	81 57·70	95 40·1	81 18	97 20·2	89 4·0	89 2·64	
25.	3 P.M.	78 56	98 30	78 1·1	99 27·5	88 43·65	79 51·4	99 12·17	80 8·72	97 21·4	89 9·42	88 56·04	
30.	3 P.M.	77 41	99 49·25	77 38·75	99 44·44	88 43·36	81 23·9	96 58·3	81 59·4	96 26·4	89 12·0	88 57·68	
April 1.	2 P.M.	76 47·1	100 2·90	78 29	100 14·20	88 53·27	81 52·4	95 48·14	81 27·12	97 13·4	89 5·27	88 59·27	
	4 P.M.	78 40·67	99 2·17	78 36·33	98 52	88 48·04	80 8·27	97 51·13	80 20·27	97 48·5	89 2·04	88 55·04	
May 28.	8 A.M.	86 31·7	92 47	86 26·83	93 30	89 48·87	73 9·5	106 11·5	84 20	94 35·8	89 34·2	89 41·53	89° 41′·9 N. Variation 57° 15′ W.; lat. 69° 34′ 45″ N.; long. 94° 53′ W.
	2 P.M.	86 17·22	92 51·3	87 2·14	93 32·16	89 55·71	74 42·2	104 58·22	83 24·7	94 50·18	89 28·83	89 42·27	
June 1.	Noon.	86 23·67	93 8·33	87 6·17	93 32·83	90 2·75	73 53·67	104 51·67	83 44·33	97 7·5	89 54·29	89 58·52	89° 58′·11 N. Assumed direction of meridian S.75° W. (True.)
	3 P.M.	85 55·5	93 32·62	86 40·67	93 54	90 0·71	73 22	105 24·83	83 23·33	97 28·67	89 54·71	89 57·71	
	5 P.M.	86 32·33	93 10·33	87 16·67	93 9·83	90 1·79	74 58·83	104 16·83	83 0·83	97 5·13	89 50·15	89 55·97	89° 59′·465 N. Assumed direction of meridian N. 60° W.; lat. 70° 5′ 17″ N.; long. 96° 45′ 48″ W.
	7 P.M.	86 52·83	93 9·67	87 14·50	93 32·0	90 12·25	74 55	104 24·5	83 38·17	96 37	89 53·67	90 2·96	
2.	9 A.M.	84 24.33	96 12·67	82 46·37	96 55·1	90 4·62	82 29·5	97 14·33	85 14·5	94 58·33	89 59·14	90 1·88	89° 59′ N. Assumed direction of meridian N.15° W.
	11 A.M.	84 3	96 25·56	82 32·60	96 35·75	89 54·08	82 36·5	97 26·75	85 20·67	94 29·0	89 58·17	89 56·12	
6.	8 A.M.	86 0·5	92 15·83	86 58·33	93 6·67	89 42·38	75 25·67	103 55·83	82 40·17	96 28·5	89 37·54	89 40·19	Same position as on the 28th of May.
8.	9 A.M.	86 27·5	92 3·33	87 13·67	92 17·83	89 30·58	75 20	101 36·17	81 22·33	98 37·83	89 14·04	89 22·33	At Cape Isabella. Lat. 69° 26′ 20″; long. 93° 51′ W.
9.	8 A.M.	84 49·83	94 33·13	84 25·67	93 42·5	89 21·03	77 48·83	100 1	81 42	97 21·5	89 13·33	89 17·18	At Padleak. Lat. 69° 30′ 1″; long. 93° 27′ 52″ W.
17.	2 P.M.	86 5	92 40·2	87 41·5	91 52·5	89 34·8	82 41	93 30	85 1·3	93 2·3	88 33·65	89 4·22	
	5 P.M.	86 9·8	91 48·5	87 45	91 15	89 14·57	80 45	96 30·8	85 9·2	93 7	88 53	89 3·79	
July 13.	2 P.M.	85 43·33	92 4·5	87 50·33	91 42	89 20·40	82 23	95 1·5	82 37	95 14·83	88 49·04	89 4·74	Observed dip at Sheriff Bay on my return from the magnetic pole 89° 3′·32 N. (1831.)
Aug. 12.	1 P.M.	86 27·5	91 41·7	87 57·5	91 14·7	89 20·35	84 9·2	93 24·2	79 45·8	97 38·3	88 44·37	89 2·36	
20.	Noon.	80 3·34	98 7·5	80 46·7	97 20·8	89 4·53	76 15·7	101 30	99 53·2	78 15	88 58·47	89 1·50	
Oct. 21.	10 A.M.	84 40·17	93 52	84 24	93 33·45	89 7·40	79 1·89	98 5·67	81 36·67	96 5	88 42·31	88 54·96	
22.	9 A.M.	84 40·5	94 16·5	84 50·12	93 49·37	89 24·12	77 29·4	99 41	80 24·5	96 45·2	88 35·03	88 59·57	
23.	Noon.	84 9	93 13·9	84 40·6	93 43·2	88 56·67	78 55·4	99 5·9	81 57·6	95 32·8	88 53·93	88 55·30	
Nov. 21.	Noon.	84 18·8	94 8·1	84 18·1	94 27·9	89 17·98	77 20·5	99 22	78 41·8	98 5·6	88 22·47	88 50·22	
22.	1 P.M.	84 56·2	93 46·6	84 54·6	93 48·6	89 21·5	78 55·3	98 24	79 28·3	96 51	88 24·65	88 53·07	
23.	1 P.M.	84 43	93 37·6	84 59	93 24·4	89 11	79 39·8	98 8	80 36·2	95 55·4	88 34·88	88 52·92	
Dec. 24.	10 A.M.	84 42·9	93 50·7	84 11·7	94 32·6	89 16·97	79 47·5	98 31	81 51·5	94 46	88 44	89 0·49	
	1 P.M.	85 21·5	93 2	84 19.8	93 57	89 10·07	79 58·6	98 16·8	81 37	94 41·5	88 38·48	88 54·27	Observed dip at Victory Harbour 88° 54′·86 N. Variation 101° 32′·03 W.; lat. 70° 9′ 18″ N.; long. 91° 30′ 33″ W.
1832.													
Jan. 21.	Noon.	85 1	94 8·6	84 24·5	94 23·2	89 29·32	76 53	100 16·2	81 7	95 59·4	88 33·9	89 1·61	
Feb. 16.	1 P.M.	84 58·5	95 11·8	83 17·3	95 14	89 40·04	76 46·1	100 52	79 51·7	96 31·1	88 30·22	89 5·31	
18.	1 P.M.	83 48·4	95 18·6	81 58·1	95 19·7	89 6·2	77 30·1	100 39·5	81 30·4	95 37·8	88 49·45	88 57·82	
March 17.	3 P.M.	83 16·2	94 41·6	82 32	95 42·1	89 2·98	74 53·9	102 45·2	78 55	96 3·1	88 9·3	88 36·14	
27.	2 P.M.	83 30·7	94 48·4	84 2·9	94 38·4	89 15·1	74 4·5	102 54·7	78 28	98 45·6	88 33·2	88 54·15	
April 13.	.....	83 38·5	94 47	82 47·6	95 14·9	89 7	75 36·9	101 13	78 53	98 23·6	88 31·62	88 49·31	
15.	.....	83 5·5	95 22·5	82 30·9	96 27·2	89 21·52	78 21	98 23·3	75 45·4	101 46·7	88 34·12	88 57·82	

IV. *Notice as to the supposed Identity of the large Mass of Meteoric Iron now in the British Museum, with the celebrated Otumpa Iron described by* Rubin de Celis *in the Philosophical Transactions for* 1786. *Communicated in a Letter from* Woodbine Parish, *Esq. F.R.S. to* Charles König, *Esq. Foreign Secretary of the Royal Society.*

Received and read November 21, 1833.

As the identity of the large mass of meteoric iron in the British Museum with the celebrated Otumpa iron, described by Rubin de Celis in the Philosophical Transactions for 1786, has been the subject of frequent inquiry, the following short historical notice, relating to that mass, is communicated by Woodbine Parish, Esq. F.R.S., by whom, when His Majesty's Chargé d'Affaires at Buenos Ayres, it was sent to England. —C. K.

"London,
August 10th, 1833.

"Dear Sir,

"Agreeably to my promise, I have taken some trouble to ascertain the precise history of the large mass of native iron which I sent home to Sir Humphry Davy from Buenos Ayres, and which is deposited in the British Museum. There is no doubt of its coming from the same place as that described by Rubin de Celis, though whether it be a fragment of that particular mass upon which he made his report, or a smaller one in its immediate vicinity, I am not able to say, for there certainly is an impression at Buenos Ayres that there is not only one, but that several masses of this iron are to be found in that part of the Gran Chaco referred to by Rubin de Celis. I was under the impression that it had been sent for in order to be forwarded to Madrid; but in this I was led into error; and I have only lately ascertained through Mr. Moreno, the Buenos Ayrean Minister, that the real history of its being at Buenos Ayres is as follows.

"After the people of that country had declared their independency of Spain, they were blockaded by a naval force, which cut off their communication with Europe, and especially prevented their receiving what they were in great need of, viz. arms and other warlike stores. In this dilemma it was suggested that muskets might be made if they had but the material; and it was then that the iron formerly described by De Celis was recollected as existing within their own territories, and people were sent to the Gran Chaco to bring away at least a part of it, that it might be ascertained how far it was fit for the purpose; and thus this particular mass was brought to Buenos Ayres. By the time it arrived there, early in 1813, the necessity for using

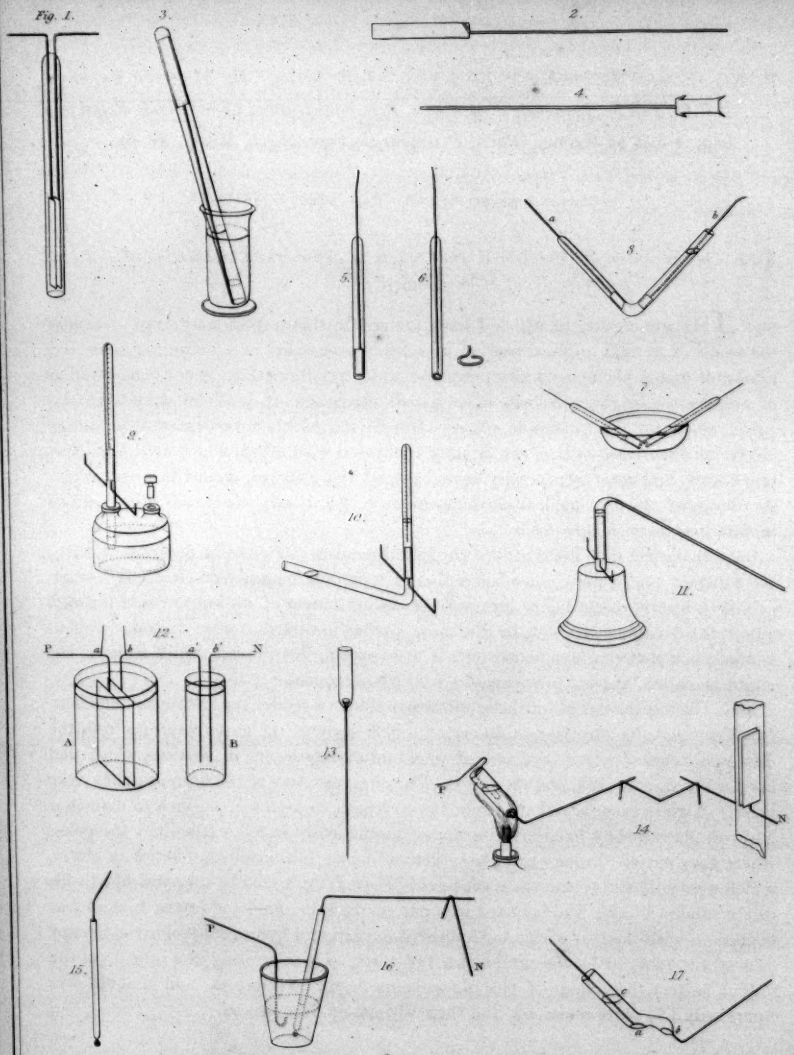
it had ceased: the projected experiment, however, was tried, and a pair of pistols were made of it, which were afterwards sent as a present to the President of the United States. The remainder of the mass brought down from the Chaco, after some specimens were taken from it, was laid aside in the arsenal at Buenos Ayres until it was given to me by the Government of that country on the occasion of my having to carry into effect the recognition by Great Britain of its political independence. And this is the precise history of this iron. If you think there is anything in it worth making known to the Royal Society, you are quite at liberty to copy or extract this account of it.

"Believe me to be, my dear Sir,

"Very truly yours,

"Woodbine Parish."

"To Charles Konig, Esq.,
British Museum."



E. Baxter sc

V. *Experimental Researches in Electricity.—Sixth Series.* By MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Copenhagen, Berlin, &c. &c.*

Received November 30, 1833,—Read January 11, 1834.

§ 12. *On the power of Metals and other Solids to induce the Combination of Gaseous Bodies.*

564. THE conclusion at which I have arrived in this section may seem to render the whole of it unfit to form part of a series of researches in electricity; since, remarkable as the phenomena are, the power which produces them is not considered as of an electric origin, otherwise than as all attraction of particles may have this subtil agent for their common cause. But as the effects investigated arose out of electrical researches, as they are directly connected with others which are of an electric nature, and must of necessity be understood and guarded against in a very extensive series of electro-chemical decompositions (707.), I have felt myself fully justified in detailing them in this place.

565. Believing that I had proved (by experiments hereafter to be described (705.),) the constant and definite chemical action of a certain quantity of electricity, whatever its intensity might be, or however the circumstances of its transmission through either the decomposing body or the more perfect conductors were varied, I endeavoured upon that result to construct a new measuring instrument, which from its use might be called, at least provisionally, a *Volta-electrometer* (739.).

566. During the course of the experiments made to render the instrument efficient, I was occasionally surprised at observing a deficiency of the gases resulting from the decompositions of water, and at last an actual disappearance of portions which had been evolved, collected, and measured. The circumstances of the disappearance were these. A glass tube, about twelve inches in length and $\frac{3}{4}$ths of an inch in diameter, had two platina poles fixed into its upper, hermetically sealed, extremity: the poles, where they passed through the glass, were of wire; but terminated below in plates, which were soldered to the wires with gold (Plate I. fig. 1.). The tube was filled with dilute sulphuric acid, and inverted in a cup of the same fluid; a voltaic battery was connected with the two wires, and sufficient oxygen and hydrogen evolved to occupy $\frac{3}{4}$ths of the tube, or by the graduation 116 parts. On separating the tube from the voltaic battery the volume of gas immediately began to diminish, and in about five hours only $13\frac{1}{3}$ parts remained, and these ultimately disappeared.

567. It was found, by various experiments, that this effect was not due to the escape or solution of the gas, nor to recombination of the oxygen or hydrogen in consequence of any peculiar condition *they* might be supposed to possess under the circumstances; but to be occasioned by the action of one or both of the poles within the tube upon the gas around them. On disuniting the poles from the pile after they had acted upon dilute sulphuric acid, and introducing them into separate tubes containing mixed oxygen and hydrogen, it was found that the *positive* pole effected the union of the gases, but the negative pole apparently not (588.). It was ascertained also that no action of a sensible kind took place between the positive pole with oxygen or hydrogen alone.

568. These experiments reduced the phenomena to the consequence of a power possessed by the platina, after it had been the positive pole of a voltaic pile, of causing the combination of oxygen and hydrogen at common, or even at low, temperatures. This effect is, as far as I am aware, altogether new, and was immediately followed out to ascertain whether it was really of an electric nature, and how far it would interfere with the determination of the quantities evolved in the cases of electro-chemical decomposition required in the fourteenth section of these Researches.

569. Several platina plates were prepared (fig. 2.). They were nearly half an inch wide, and two inches and a half long: some were $\frac{1}{500}$dth of an inch, others not more than $\frac{1}{500}$dth, and some were as much as $\frac{1}{70}$th of an inch in thickness. Each had a piece of platina wire, about seven inches long, soldered to it by pure gold. Then a number of glass tubes were prepared: they were about nine or ten inches in length, $\frac{1}{3}$ths of an inch in internal diameter, were sealed hermetically at one extremity, and were graduated. Into these tubes was put a mixture of two volumes of hydrogen and one of oxygen, at the water pneumatic trough, and when one of the plates described had been connected with the positive or negative pole of the voltaic battery for a given time, or had been otherwise prepared, it was introduced through the water into the gas within the tube; the whole set aside in a test-glass (fig. 3.), and left for a longer or shorter period, that the action might be observed.

570. The following result may be given as an illustration of the phenomenon to be investigated. Diluted sulphuric acid, of the specific gravity 1·336, was put into a glass jar, in which was placed also a large platina plate, connected with the negative end of a voltaic battery of forty pairs of four-inch plates, with double coppers, and moderately charged. One of the plates (569.) was then connected with the positive extremity, and immersed in the same jar of acid for five minutes, after which it was separated from the voltaic battery, washed in distilled water, and introduced through the water of the pneumatic trough into a tube containing the mixture of oxygen and hydrogen (569.). The volume of gases immediately began to lessen, the diminution proceeding more and more rapidly until about $\frac{3}{4}$ths of the mixture had disappeared. The upper end of the tube became quite warm, the plate itself so hot that the water boiled as it rose over it; and in less than a minute a cubical inch and a half of the

gases were gone, having been combined by the power of the platina, and converted into water.

571. This extraordinary influence acquired by the platina at the positive pole of the pile, is exerted far more readily and effectively on oxygen and hydrogen than on any other mixture of gases that I have tried. One volume of nitrous gas was mixed with a volume of hydrogen, and introduced into a tube with a plate which had been made positive in the dilute sulphuric acid for four minutes (570.). There was no sensible action in an hour: being left for thirty-six hours, there was a diminution of about one eighth of the whole volume. Action had taken place, but it had been very feeble.

572. A mixture of two volumes of nitrous oxide with one volume of hydrogen was put with a plate similarly prepared into a tube (569. 570.). This also showed no action immediately; but in thirty-six hours nearly a fourth of the whole had disappeared, i. e. about half of a cubic inch. By comparison with another tube containing the same mixture without a plate, it appeared that a part of the diminution was due to solution, and another part to the power of the platina; but the action had been very slow and feeble.

573. A mixture of one volume olefiant gas and three volumes oxygen was not affected by such a platina plate, even though left together for several days (640. 641.).

574. A mixture of two volumes carbonic oxide and one volume oxygen was also unaffected by the prepared platina plate in several days (645. &c.).

575. A mixture of equal volumes of chlorine and hydrogen was used in several experiments, with plates prepared in a similar manner (570.). Diminution of bulk soon took place; but when after thirty-six hours the experiments were examined, it was found that nearly all the chlorine had disappeared, having been absorbed, principally by the water, and that the original volume of hydrogen remained unchanged. No combination of the gases, therefore, had here taken place.

576. Reverting to the action of the prepared plates on mixtures of oxygen and hydrogen (570.), I found that the power, though gradually diminishing in all cases, could still be retained for a period varying in its length with circumstances. When tubes containing plates (569.) were supplied with fresh portions of mixed oxygen and hydrogen as the previous portions were condensed, the action was found to continue for above thirty hours, and in some cases slow combination could be observed even after eighty hours; but the continuance of the action greatly depended upon the purity of the gases used (638.).

577. Some plates (569.) were made positive for four minutes in dilute sulphuric acid of specific gravity 1·336: they were rinsed in distilled water, after which two were put into a small bottle and closed up, whilst others were left exposed to the air. The plates preserved in the limited portion of air were found to retain their power after eight days, but those exposed to the atmosphere had lost their force almost entirely in twelve hours, and in some situations, where currents existed, in a much shorter time.

578. Plates were made positive for five minutes in sulphuric acid, specific gra-

vity 1·336. One of these was retained in similar acid for eight minutes after separation from the battery: it then acted on mixed oxygen and hydrogen with apparently undiminished vigour. Others were left in similar acid for forty hours, and some even for eight days, after the electrization, and then acted as well in combining oxygen and hydrogen gas as those which were used immediately after electrization.

579. The effect of a caustic solution of potassa in preserving the platina plates was tried in a similar manner. After being retained in such a solution for forty hours, they acted exceedingly well on oxygen and hydrogen, and one caused such rapid condensation of the gases, that the plate became much heated, and I expected the temperature would have risen to ignition.

580. When similarly prepared plates (569.) had been put into distilled water for forty hours, and then introduced into mixed oxygen and hydrogen, they were found to act but very slowly and feebly as compared with those which had been preserved in acid or alkali. When, however, the quantity of water was but small, the power was very little impaired after three or four days. As the water had been retained in a wooden vessel, portions of it were redistilled in glass, and this was found to preserve prepared plates for a great length of time. Prepared plates were put into tubes with this water and closed up; some of them, taken out at the end of twenty-four days, were found very active on mixed oxygen and hydrogen; others, which were left in the water for fifty-three days, were still found to cause the combination of the gases. The tubes had been closed only by corks.

581. The act of combination always seemed to diminish, or apparently exhaust, the power of the platina plate. It is true that in most, if not all instances, the combination of the gases, at first insensible, gradually increased in rapidity, and sometimes reached to explosion; but when the latter did not happen, the rapidity of combination diminished; and although fresh mixtures of gas were introduced into the tubes, the combination went on more and more slowly, and at last ceased altogether. The first effect of an increase in the rapidity of combination depended in part upon the water flowing off from the platina plate, and allowing a better contact with the gas, and in part upon the heat evolved during the progress of the combination (630.). But notwithstanding the effect of these causes, diminution, and at last cessation of the power, always occurred. It must not, however, be unnoticed, that the purer the gases subjected to the action of the plate, the longer was its combining power retained. With the mixture evolved at the poles of the voltaic pile in pure dilute sulphuric acid, it continued longest; and with oxygen and hydrogen, of perfect purity, it probably would not be diminished at all.

582. Different modes of treatment applied to the platina plate, after it had ceased to be the positive pole of the pile, affected its power very curiously. A plate which had been a positive pole in diluted sulphuric acid of specific gravity 1·336 for four or five minutes, if rinsed in water and put into mixed oxygen and hydrogen, would act very well, and condense perhaps one cubic inch and a half of gas in six or seven

minutes; but if that same plate, instead of being merely rinsed, had been left in distilled water for twelve or fifteen minutes, or more, it would rarely fail, when put into the oxygen and hydrogen, of becoming, in the course of a minute or two, ignited, and would generally explode the gases. Occasionally the time occupied in bringing on the action extended to eight or nine minutes, and sometimes even to forty minutes, and yet ignition and explosion would result. This effect is due to the removal of a portion of acid which otherwise adheres firmly to the plate.

583. Occasionally the platina plates (569.), after being made the positive pole of the battery, were washed, wiped with filtering-paper or a cloth, and washed and wiped again. Being then introduced into mixed oxygen and hydrogen, they acted apparently as if they had been unaffected by the treatment. Sometimes the tubes containing the gas were opened in the air for an instant, and the plates put in dry; but no sensible difference in action was perceived, except that it commenced sooner.

584. The power of heat in altering the action of the prepared platina plates was also tried (595.). Plates which had been rendered positive in dilute sulphuric acid for four minutes were well washed in water, and heated to redness in the flame of a spirit-lamp: after this they acted very well on mixed oxygen and hydrogen. Others, which had been heated more powerfully by the blowpipe, acted afterwards on the gases, though not so powerfully as the former. Hence it appears that heat does not take away the power acquired by the platina at the positive pole of the pile: the occasional diminution of force seemed always referable to other causes than the mere heat. If, for instance, the plate had not been well washed from the acid, or if the flame used was carbonaceous, or was that of an alcohol lamp trimmed with spirit containing a little acid, or having a wick on which salt, or other extraneous matter, had been placed, then the power of the plate was quickly and greatly diminished (634. 636.).

585. This remarkable property was conferred upon platina when it was made the positive pole in sulphuric acid of specific gravity 1·336, or when it was considerably weaker, or when stronger, even up to the strength of oil of vitriol. Strong and dilute nitric acid, dilute acetic acid, solutions of tartaric, citric, and oxalic acids, were used with equal success. When muriatic acid was used, the plates acquired the power of condensing the oxygen and hydrogen, but in a much inferior degree.

586. Plates which were made positive in solution of caustic potassa did not show any sensible action upon the mixed oxygen and hydrogen. Other plates made positive in solutions of carbonates of potassa and soda exhibited the action, but only in a feeble degree.

587. When a neutral solution of sulphate of soda, or of nitre, or of chlorate of potassa, or of phosphate of potassa, or acetate of potassa, or sulphate of copper, was used, the plates, rendered positive in them for four minutes, and then washed in water, acted very readily and powerfully on the mixed oxygen and hydrogen.

588. It became a very important point, in reference to the *cause* of this action of

the platina, to determine whether the *positive* pole *only* could confer it (567.), or whether, notwithstanding the numerous contrary cases, the *negative* pole might not have the power when such circumstances as could interfere with or prevent the action were avoided. Three plates were therefore rendered negative for four minutes in diluted sulphuric acid of specific gravity 1·336, washed in distilled water, and put into mixed oxygen and hydrogen. *All* of them *acted*, though not so strongly as they would have done if they had been rendered positive. Each combined about a cubical inch and a quarter of the gases in twenty-five minutes. On every repetition of the experiment the same result was obtained; and when the plates were retained in distilled water for ten or twelve minutes, before being introduced into the gas (582.), the action was very much quickened.

589. But when there was any metallic or other substance present in the acid, which could be precipitated on the negative plate, then that plate ceased to act upon the mixed oxygen and hydrogen.

590. These experiments led to the expectation that the power of causing oxygen and hydrogen to combine, which could be conferred upon any piece of platina by making it the positive pole of a voltaic pile, was not essentially dependent upon the action of the pile, or upon any structure or arrangement of parts it might receive whilst in association with it, but belonged to the platina *at all times*, and was *always effective* when the surface was *perfectly clean*. And though, when made the *positive* pole of the pile in acids, the circumstances might well be considered as those which would cleanse the surface of the platina in the most effectual manner, it did not seem impossible that ordinary operations should produce the same result, although in a less eminent degree.

591. Accordingly, a platina plate (569.) was cleaned by being rubbed with a cork, a little water, and some coal-fire ashes upon a glass plate: being washed, it was put into mixed oxygen and hydrogen, and was found to act at first slowly, and then more rapidly. In an hour, a cubical inch and a half had disappeared.

592. Other plates were cleaned with ordinary sand-paper and water; others with whitening and water; others with emery and water; others, again, with black oxide of manganese and water; and others with a piece of charcoal and water. All of these acted in tubes of oxygen and hydrogen, causing combination of the gases. The action was by no means so powerful as that produced by plates having been in communication with the battery; but from one to two cubical inches of the gases disappeared, in periods extending from twenty-five to eighty or ninety minutes.

593. Upon cleaning the plates with a cork, ground emery, and dilute sulphuric acid, they were found to act still better. In order to simplify the conditions, the cork was dismissed, and a piece of platina foil used instead; still the effect took place. Then the acid was dismissed, and a solution of *potassa* used, but the effect occurred as before.

594. These results are abundantly sufficient to show that the mere mechanical

cleansing of the surface of the platina is sufficient to enable it to exert its combining power over oxygen and hydrogen at common temperatures.

595. I now tried the effect of heat in conferring this property upon platina (584.). Plates which had no action on the mixture of oxygen and hydrogen were heated by the flame of a freshly trimmed spirit-lamp, urged by a mouth blowpipe, and when cold were put into tubes of the mixed gases: they acted slowly at first, but after two or three hours condensed nearly all the gases.

596. A plate of platina, which was about one inch wide and two and three quarters in length, and which had not been used in any of the preceding experiments, was curved a little so as to enter a tube, and left in a mixture of oxygen and hydrogen for thirteen hours: not the slightest action or combination of the gases occurred. It was withdrawn at the pneumatic trough from the gas through the water, heated red hot by the spirit-lamp and blowpipe, and then returned when cold into the *same* portion of gas. In the course of a few minutes diminution of the gases could be observed, and in forty-five minutes about one cubical inch and a quarter had disappeared. In many other experiments platina plates when heated were found to acquire the power of combining oxygen and hydrogen.

597. But it happened not unfrequently that plates, after being heated, showed no power of combining oxygen and hydrogen gases, though left undisturbed in them for two hours. Sometimes also it would happen that a plate which, having been heated to dull redness, acted feebly, upon being heated to whiteness ceased to act; and at other times a plate which, having been slightly heated, did not act, was rendered active by a more powerful ignition.

598. Though thus uncertain in its action, and though often diminishing the power given to the plates at the positive pole of the pile (584.), still it is evident that heat can render platina active, which before was inert (595.). The cause of its occasional failure appears to be due to the surface of the metal becoming soiled, either from something previously adhering to it, which is made to adhere more closely by the action of the heat, or from matter communicated from the flame of the lamp, or from the air itself. It often happens that a polished plate of platina, when heated by the spirit-lamp and a blowpipe, becomes dulled and clouded on its surface by something either formed or deposited there; and this, and much less than this, is sufficient to prevent it from exhibiting the curious power now under consideration (634. 636.). Platina also has been said to combine with carbon; and it is not at all unlikely that in processes of heating, where carbon or its compounds are present, a film of such a compound may be thus formed, and thus prevent the exhibition of the properties belonging to *pure* platina.

599. The action of alkalies and acids in giving platina this property was now experimentally examined. Platina plates (569.) having no action on mixed oxygen and hydrogen, being boiled in a solution of caustic potassa, washed, and then put into the gases, were found occasionally to act pretty well, but at other times to fail. In

the latter case I concluded that the impurity upon the surface of the platina was of a nature not to be removed by the mere solvent action of the alkali, for when the plates were rubbed with a little emery, and the same solution of alkali (592.), they became active.

600. The action of acids was far more constant and satisfactory. A platina plate was boiled in dilute nitric acid: being washed and put into mixed oxygen and hydrogen gases, it acted well. Other plates were boiled in strong nitric acid for periods extending from half a minute to four minutes, and then being washed in distilled water, were found to act very well, condensing one cubic inch and a half of gas in the space of eight or nine minutes, and rendering the tube warm (570.).

601. Strong sulphuric acid was very effectual in rendering the platina active. A plate (569.) was heated in it for a minute, then washed and put into the mixed oxygen and hydrogen, upon which it acted as well as if it had been made the positive pole of a voltaic pile (570.).

602. Plates which, after being heated or electrized in alkali, or after other treatment, were found inert, immediately received power by being dipped for a minute or two, or even only for an instant, into hot oil of vitriol, and then into water.

603. When the plate was dipped into the oil of vitriol, taken out, and then heated so as to drive off the acid, it did not act, in consequence of the impurity left by the acid upon its surface.

604. Vegetable acids, as acetic and tartaric, sometimes rendered inert platina active, at other times not. This, I believe, depended upon the character of the matter previously soiling the plates, and which may easily be supposed to be sometimes of such a nature as to be removed by these acids, and at other times not. Weak sulphuric acid showed the same difference, but strong sulphuric acid (601.) never failed in its action.

605. The most favourable treatment, excepting that of making the plate a positive pole in strong acid, was as follows. The plate was held over a spirit-lamp flame, and when hot, rubbed with a piece of potassa fusa (caustic potash), which melting, covered the metal with a coat of very strong alkali, and this was retained fused upon the surface for a second or two*: it was then put into water for four or five minutes to wash off the alkali, shaken, and immersed for about a minute in hot strong oil of vitriol; from this it was removed into distilled water, where it was allowed to remain ten or fifteen minutes to remove the last traces of acid (582.). Being then put into a mixture of oxygen and hydrogen, combination immediately began, and proceeded rapidly; the tube became warm, the platina became red hot, and the residue of the gases was inflamed. This effect could be repeated at pleasure, and thus the maximum phenomenon could be produced without the aid of the voltaic battery.

606. When a solution of tartaric or acetic acid was substituted, in this mode of preparation, for the sulphuric acid, still the plate was found to acquire the same power,

* The heat need not be raised so much as to make the alkali tarnish the platina, although if that effect does take place it does not prevent the ultimate action.

and would often produce explosion in the mixed gases; but the strong sulphuric acid was most certain and powerful.

607. If borax, or a mixture of the carbonates of potash and soda, be fused on the surface of a platina plate, and that plate be well washed in water, it will be found to have acquired the power of combining oxygen and hydrogen, but only in a moderate degree; but if, after the fusion and washing, it be dipped in the hot sulphuric acid (601.), it will become very active.

608. Other metals than platina were then experimented with. Gold and palladium exhibited the power either when made the positive pole of the voltaic battery (570.), or when acted on by hot oil of vitriol (601.). When palladium is used, the action of the battery or acid should be moderated, as that metal is soon acted upon. Silver and copper could not be made to show any effect at common temperatures.

609. There can remain no doubt that the property of inducing combination, which can thus be conferred upon masses of platina and other metals by connecting them with the poles of the battery, or by cleansing processes either of a mechanical or chemical nature, is the same as that which was discovered by Dobereiner*, in 1823, to belong in so eminent a degree to spongy platina, and which was afterwards so well experimented upon and illustrated by MM. Dulong and Thenard†, in 1823. The latter philosophers even quote experiments in which a very fine platina wire, which had been coiled up and digested in nitric, sulphuric, or muriatic acid, became ignited when put into a jet of hydrogen gas‡. This effect I can now produce at pleasure with either wires or plates by the processes described (570. 601. 605.); and by using a smaller plate cut so that it shall rest against the glass by a few points, and yet allow the water to flow off (fig. 4.), the loss of heat is less, the metal is assimilated somewhat to the spongy state, and the probability of failure almost entirely removed.

610. M. Dobereiner refers the effect entirely to an electric action. He considers the platina and hydrogen as forming a voltaic element of the ordinary kind, in which the hydrogen, being very highly positive, represents the zinc of the usual arrangement, and like it, therefore, attracts oxygen and combines with it§.

611. In the two excellent experimental papers by MM. Dulong and Thenard‖, those philosophers show that elevation of temperature favours the action, but does not alter its character, Sir Humphry Davy's incandescent platina wire being the same phenomenon with Dobereiner's spongy platina. They show that *all* metals have this power in a greater or smaller degree, and that it is even possessed by such bodies as charcoal, pumice, porcelain, glass, rock crystal, &c., when their temperatures are raised; and that another of Davy's effects, in which oxygen and hydrogen had combined slowly together at a heat below ignition, was really dependent upon the pro-

* Annales de Chimie, tom. xxiv. p. 93.

† Ibid. tom. xxiii. p. 440.; tom. xxiv. p. 380.

‡ Ibid. tom. xxiv. p. 383.

§ Ibid. tom. xxiv. pp. 94, 95. Also Bibliothéque Universelle, tom. xxiv. p. 54.

‖ Annales de Chimie, tom. xxiii. p. 440.; tom. xxiv. p. 380.

perty of the heated glass, which it has in common with the bodies named above. They state that liquids do not show this effect, at least that mercury, at or below the boiling point, has not the power; that it is not due to porosity; that the same body varies very much in its action, according to its state; and that many other gaseous mixtures besides oxygen and hydrogen are affected, and made to act chemically, when the temperature is raised. They think it probable that spongy platina acquires its power from contact with the acid evolved during its reduction, or from the heat itself to which it is then submitted.

612. MM. Dulong and Thenard express themselves with great caution on the theory of this action; but, referring to the decomposing power of metals on ammonia when heated to temperatures not sufficient alone to affect the alkali. They remark that those metals which in this case are most efficacious, are the least so in causing the combination of oxygen and hydrogen; whilst platina, gold, &c., which have least power of decomposing ammonia, have most power of combining the elements of water; from which they are led to believe, that amongst gases, some tend to unite under the influence of metals, whilst others tend to separate, and that this property varies in opposite directions with the different metals. At the close of their second paper they observe, that the action is of a kind that cannot be connected with any known theory; and though it is very remarkable that the effects are transient, like those of most electrical actions, yet they state that the greater number of the results observed by them are inexplicable, by supposing them to be of a purely electric origin.

613. Dr. Fusinieri has also written on this subject, and given a theory which he considers as sufficient to account for the phenomena*. He expresses the immediate cause thus: "The platina determines upon its surface a continual renovation of *concrete laminæ* of the combustible substance of the gases or vapours, which flowing over it, are burnt, pass away, and are renewed: this combustion at the surface raises and sustains the temperature of the metal." The combustible substance, thus reduced into imperceptible laminæ, of which the concrete parts are in contact with the oxygen, is presumed to be in a state combinable with the oxygen at a much lower temperature than when it is in the gaseous state, and more in analogy with what is called the nascent condition. That combustible gases should lose their elastic state, and become concrete, assuming the form of exceedingly attenuated but solid strata, is considered as proved by facts, some of which are quoted in the Giornale di Fisica for 1824†; and though the theory requires that they should assume this state at high temperatures, and though the *similar* films of aqueous and other matter are dissipated by the action of heat, still the facts are considered as justifying the conclusion against all opposition of reasoning.

614. The power or force which makes combustible gas or vapour abandon its elastic state in contact with a solid, that it may cover the latter with a thin stratum of its own proper substance, is considered as being neither attraction nor affinity. It

* Giornale di Fisica, &c., 1825, tom. viii. p. 259. † pp. 138, 371.

is able also to extend liquids and solids in concrete laminæ over the surface of the acting solid body, and consists in a *repulsion*, which is developed from the parts of the solid body by the simple fact of attenuation, and is highest when the attenuation is most complete. The force has a progressive development, and acts most powerfully, or at first, in the direction in which the dimensions of the attenuated mass decrease, and then in the direction of the angles or corners which from any cause may exist on the surface. This force not only causes spontaneous diffusion of gases and other substances over the surface, but is considered as very elementary in its nature, and competent to account for all the phenomena of capillarity, chemical affinity, attraction of aggregation, rarefaction, ebullition, volatilization, explosion, and other thermometric effects, as well as inflammation, detonation, &c. &c. It is considered as a form of heat, to which the term *native caloric* is given, and is still further viewed as the principle of the two electricities and the two magnetisms.

615. I have been the more anxious to give a correct abstract of Dr. FUSINIERI's view, both because I cannot form a distinct idea of the power to which he refers the phenomena, and because of my imperfect knowledge of the language in which the memoir is written. I would therefore beg to refer those who pursue the subject to the memoir itself.

616. Not feeling, however, that the problem has yet been solved, I venture to give the view which seems to me sufficient, upon known principles, to account for the effect.

617. It may be observed of this action, that, with regard to platina, it cannot be due to any peculiar, temporary condition, either of an electric or of any other nature: the activity of plates rendered either positive or negative by the pole, or cleaned with such different substances as acids, alkalies, or water; charcoal, emery, ashes, or glass; or merely heated, is sufficient to negative such an opinion. Neither does it depend upon the spongy and porous, or upon the compact and burnished, or upon the massive or the attenuated state of the metal, for in any of these states it may be rendered effective, or its action may be taken away. The only essential condition appears to be a *perfectly clean* and *metallic surface*, for whenever that is provided the platina acts, whatever its form and condition in other respects may be; and though variations in the latter points will very much affect the rapidity, and therefore the visible appearances and secondary effects, of the action, i. e. the ignition of the metal and the inflammation of the gases, they, even in their most favourable state, cannot produce any effect unless the condition of a clean, pure, metallic surface be also fulfilled.

618. The effect is evidently produced by most, if not all, solid bodies, weakly perhaps by many of them, but rising to a high degree in platina. DULONG and THENARD have very philosophically extended our knowledge of the property to its possession by all the metals, and by earths, glass, stones, &c. (611.); and every idea of its being a known and recognised electric action is in this way removed.

619. All the phenomena connected with this subject press upon my mind the conviction that the effects in question are entirely incidental and of a secondary nature;

that they are dependent upon the *natural conditions* of gaseous elasticity combined with the exertion of that attractive force, possessed by many bodies in an eminent degree, and probably belonging to all, by which they are drawn into association more or less close, without at the same time undergoing chemical combination, though often assuming the condition of adhesion; and which occasionally leads, under very favourable circumstances, as in the present instance, to the combination of bodies simultaneously subjected to this attraction. I am prepared myself to admit (and probably many others are of the same opinion), both with respect to the attraction of aggregation and of chemical affinity, that the sphere of action of particles extends beyond those other particles with which they are immediately and evidently in union, and in many cases produces effects rising into considerable importance: and I think that this kind of attraction is a determining cause of DOBEREINER's effect, and of the many others of a similar nature.

620. Bodies which become wetted by fluids with which they do not combine chemically, or in which they do not dissolve, are simple and well known instances of this kind of attraction.

621. All those cases of bodies which being insoluble in water and not combining with it are hygrometric, and condense its vapour around or upon their surface, are stronger instances of the same power, and approach a little nearer to the cases under investigation. If pulverised clay, protoxide or peroxide of iron, oxide of manganese, charcoal, or even metals, as spongy platina or precipitated silver, be put into an atmosphere containing vapour of water, they soon become moist by virtue of an attraction which is able to condense the vapour upon, although not to combine it with, the substances; and if, as is well known, these bodies so damped be put into a dry atmosphere, as, for instance, one confined over sulphuric acid, or if they be heated, then they yield up this water again almost entirely, it not being in direct or permanent combination*.

622. Still better instances of the power I refer to, because they are more analogous to the cases to be explained, are furnished by the attraction existing between glass and air, so well known to barometer and thermometer makers, for here the adhesion or attraction is exerted between a solid and gases, bodies having very different physical conditions, having no power of combination with each other, and each retaining, during the time of action, its physical state unchanged†. When mercury is poured into a barometer tube, a film of air will remain between the metal and glass for months, or, as far as is known, for years, for it has never been displaced except by the

* I met at Edinburgh with a remarkable case as to its extent of hygrometric action, assisted a little perhaps by very slight solvent power. Some turf had been well dried by long exposure in a covered place to the atmosphere, but being then submitted to the action of a hydrostatic press, it yielded, *by the mere influence of the pressure*, 54 per cent. of water.

† FUSINIERI and BELLANI consider the air as forming solid concrete films in these cases.—Giornale di Fisica, tom. viii. p. 262. 1825.

action of means especially fitted for the purpose. These consist in boiling the mercury, or, in other words, of forming an abundance of vapour, which coming in contact with every part of the glass and every portion of surface of the mercury, gradually mingles with, dilutes, and carries off the air attracted by, and adhering to, those surfaces, replacing it by other vapour, subject to an equal or perhaps greater attraction, but which when cooled condenses into the same liquid as that with which the tube is filled.

623. Extraneous bodies, which, acting as nuclei in crystallizing or depositing solutions, cause deposition of substances on them, when it does not occur elsewhere in the liquid, seem to produce their effects by a power of the same kind, i. e. a power of attraction extending to neighbouring particles, and causing them to become attached to the nuclei, although it is not strong enough to make them combine chemically with their substance.

624. It would appear from many cases of nuclei in solutions, and from the effects of bodies put into atmospheres containing the vapours of water, or camphor, or iodine, &c., as if this attraction were in part elective, partaking in its characters both of the attraction of aggregation and chemical affinity: nor is this inconsistent with, but agreeable to, the idea entertained, that it is the power of particles acting, not upon others with which they can immediately and intimately combine, but upon such as are either more distantly situated with respect to them, or which, from previous condition, physical constitution, or feeble relation, are unable to enter into decided union with them.

625. Then, of all bodies, the gases are those which might be expected to show some *mutual* action whilst *jointly* under the attractive influence of the platina or other solid acting substance. Liquids, such as water, alcohol, &c., are in so dense and comparatively incompressible a state, as to favour no expectation that their particles should approach much closer to each other by the attraction of the body to which they adhere, and yet that attraction must (according to its effects) place their particles as near to those of the solid wetted body as they are to each other, and in many cases it is evident that the former attraction is the stronger. But gases and vapours are bodies competent to suffer very great changes in the relative distances of their particles by external agencies; and where they are in immediate contact with the platina, the approximation of the particles to those of the metal may be very great. In the case of the hygrometric bodies referred to (621.), it is sufficient to reduce the vapour to the fluid state, frequently from atmospheres so rare that without this influence it would be needful to compress them by mechanical force into a bulk not more than $\frac{1}{10}$th or even $\frac{1}{20}$th of their original volume before the vapours would become liquids.

626. Another most important consideration in relation to this action of bodies, and which, as far as I am aware, has not hitherto been noticed, is the condition of elasticity under which the gases are placed against the acting surface. We have but very imperfect notions of the real and intimate conditions of the particles of a body exist-

ing in the solid, the liquid, and the gaseous state; but when we speak of the gaseous state as being due to the mutual repulsions of the particles or of their atmospheres, although we may err in imagining each particle to be a little nucleus to an atmosphere of heat, or electricity, or any other agent, we are still not likely to be in error in considering the elasticity as dependent on *mutuality* of action. Now this mutual relation fails altogether on the side of the gaseous particles next to the platina, and we might be led to expect *à priori* a deficiency of elastic force there to at least one half; for if, as DALTON has shown, the elastic force of the particles of one gas cannot act against the elastic force of the particles of another, the two being as vacua to each other, so is it far less likely that the particles of the platina can exert any influence on those of the gas against it, such as would be exerted by gaseous particles of its own kind.

627. But the diminution of power to one half on the side of the gaseous body towards the metal is only a slight result of what seems to me to flow as a necessary consequence of the known constitution of gases. An atmosphere of one gas or vapour, however dense or compressed, is in effect as a vacuum to another: thus, if a little water were put into a vessel containing a dry gas, as air, of the pressure of one hundred atmospheres, as much vapour of the water would *rise* as if it were in a perfect vacuum. Here the particles of watery vapour appear to have no difficulty in approaching within any distance of the particles of air, being influenced solely by relation to particles of their own kind; and if it be so with respect to a body having the same elastic powers as itself, how much more surely must it be so with particles, like those of the platina, or other limiting body, which, at the same time that they have not these elastic powers, are also unlike it in nature. Hence it would seem to result that the particles of hydrogen or any other gas or vapour which are next to the platina, &c., must be in such contact with it as if they were in the liquid state, and therefore almost infinitely closer to it than they are to each other, even though the metal be supposed to exert no attractive influence over them.

628. A third and very important consideration in favour of the mutual action of gases under these circumstances is their perfect miscibility. If fluid bodies capable of combining together are also capable of mixture, *they do combine* when they are mingled, not waiting for any other determining circumstance; but if two such gases as oxygen and hydrogen are put together, though they are elements having such powerful affinity as to unite naturally under a thousand different circumstances, they do not combine by mere mixture. Still it is evident that, from their perfect association, the particles are in the most favourable state possible for combination, upon the supervention of any determining cause, such either as the negative action of the platina in suppressing or annihilating, as it were, their elasticity on its side; or the positive action of the metal in condensing them against its surface by an attractive force; or the influence of both together.

629. Although there are not many distinct cases of combination under the influence of forces external to the combining particles, yet there are sufficient to remove

any difficulty which might arise on that ground. Sir JAMES HALL found carbonic acid and lime to remain combined under pressure at temperatures at which they would not have remained combined if the pressure had been removed; and I have had occasion to observe a case of direct combination in chlorine*, which being compressed at common temperatures will combine with water, and form a definite crystalline hydrate, incapable either of being formed or of existing if that pressure be removed.

630. The course of events when platina acts upon, and combines oxygen and hydrogen, may be stated, according to these principles, as follows. From the influence of the circumstances mentioned (619. &c.), i. e. the deficiency of elastic power and the attraction of the metal for the gases, the latter, when they are in association with the former, are so far condensed as to be brought within the action of their mutual affinities at the existing temperature; the deficiency of elastic power, not merely subjecting them more closely to the attractive influence of the metal, but also bringing them into a more favourable state for union, by abstracting a part of that power (upon which depends their elasticity,) which elsewhere in the mass of gases is opposing their combination. The consequence of their combination is the production of the vapour of water and an elevation of temperature. But as the attraction of the platina for the water formed is not greater than for the gases, if so great, (for the metal is scarcely hygrometric,) the vapour is quickly diffused through the remaining gases; fresh portions of the latter, therefore, come into juxtaposition with the metal, combine, and the vapour formed is also diffused, allowing new portions of gas to be acted upon. In this way the process advances, but is accelerated by the evolution of heat, which is known by experiment to facilitate the combination in proportion to its intensity, and the temperature is thus gradually exalted until ignition results.

631. The dissipation of the vapour produced at the surface of the platina, and the contact of fresh oxygen and hydrogen with the metal, form no difficulty in this explication. The platina is not considered as causing the combination of any particles with itself, but only associating them closely around it; and the compressed particles are as free to move from the platina, being replaced by other particles, as a portion of dense air upon the surface of the globe, or at the bottom of a deep mine, is free to move by the slightest impulse into the upper and rarer parts of the atmosphere.

632. It can hardly be necessary to give any reasons why platina does not show this effect under ordinary circumstances. It is then not sufficiently clean (617.), and the gases are prevented from touching it, and suffering that degree of effect which is needful to commence their combination at common temperatures, and which they can only experience at its surface. In fact, the very power which causes the combination of oxygen and hydrogen is competent, under the usual casual exposure of platina, to condense extraneous matters upon its surface, which soiling it, take away for the time its power of combining oxygen and hydrogen by preventing their contact with it.

* Philosophical Transactions, 1823, p. 161.

633. Clean platina, by which I mean such as has been made the positive pole of a pile (570.), or has been treated with acid (605.), and has then been put into distilled water for twelve or fifteen minutes, has a peculiar friction when one piece is rubbed against another. It wets freely with pure water, even after it has been shaken and dried by the heat of a spirit-lamp; and if made the pole of a voltaic pile in a dilute acid, it evolves minute bubbles from every part of its surface. But platina in its common state wants that peculiar friction: it will not wet freely with water as the clean platina does; and when made the positive pole of a pile, it for a time gives off large bubbles, which seem to cling or adhere to the metal, and are evolved at distinct and separate points of the surface. These appearances and effects, as well as its want of power on oxygen and hydrogen, are the consequences, and the indications, of a soiled surface.

634. I found also that platina plates which had been cleaned perfectly soon became soiled by mere exposure to the air; for after twenty-four hours they no longer moistened freely with water, but the fluid ran up into portions, leaving part of the surface bare, whilst other plates which had been retained in water for the same time, when they were dried (580.) did moisten, and gave the other indications of a clean surface.

635. Nor was this the case with platina or metals only, but also with earthy bodies. Rock crystal and obsidian would not wet freely upon the surface, but being moistened with strong oil of vitriol, then washed, and left in distilled water to remove all the acid, they did freely become moistened, whether they were previously dry or whether they were left wet; but being dried and left exposed to the air for twenty-four hours, their surface became so soiled that water would not then adhere freely to it, but ran up into partial portions. Wiping with a cloth (even the cleanest) was still worse than exposure to air; the surface either of the minerals or metals immediately became as if it were slightly greasy. The floating of small particles of metals under ordinary circumstances is due to the effect of this kind of soiled surface. The extreme difficulty of cleaning the surface of mercury when it has once been soiled or greased, is due to the same cause.

636. The same reasons explain why the power of the platina plates in some circumstances soon disappear, and especially upon use, and MM. Dulong and Thenard have observed the same effect with the spongy metal*, as indeed have all those who have used Dobereiner's instantaneous light machines. If left in the air, if put into ordinary distilled water, if made to act upon ordinary oxygen and hydrogen, they can still find in all these cases *that* minute portion of impurity which, when once in contact with its surface, is retained there, and is sufficient to prevent its full action upon oxygen and hydrogen at common temperatures: a slight elevation of temperature is again sufficient to compensate for their effect, and cause combination.

637. No state of things can be conceived more favourable for the production of this effect than that which is possessed by platina obtained from the ammonio-muriate by

* Annales de Chimie, tom. xxiv. p. 386.

heat: its surface is most extensive and pure, yet very accessible to the gases brought in contact with it. If placed in impurity, the interior, as THENARD and DULONG have observed, is preserved clean by the exterior; and as regards heat, it is so bad a conductor, because of its divided condition, that almost all which is evolved by the combination of the first portions of gas is retained within the mass, exalting the tendency of the succeeding portions to combine.

638. I have now to notice some very extraordinary interferences with this phenomenon, dependent, not upon the nature or condition of the metal, or other acting solid, but upon the presence of certain substances mingled with the gases acted upon; and as I shall have occasion to speak frequently of a mixture of oxygen and hydrogen, I wish it always to be understood that I mean a mixture composed of one volume oxygen to two volumes of hydrogen, being the proportions that form water. Unless otherwise expressed, the hydrogen was always that obtained by the action of dilute sulphuric acid on pure zinc, and the oxygen that obtained by the action of heat from the chlorate of potassa.

639. Mixtures of oxygen and hydrogen with *air*, containing one fourth, one half, and even two thirds of the latter, being introduced with prepared platina plates (570. 605.) into tubes, were acted upon almost as well as if no air were present: the retardation was far less than might have been expected from the mere dilution and consequent obstruction to the access of gas. In two hours and a half nearly all the oxygen and hydrogen introduced as mixture was gone.

640. But when similar experiments were made with *olefiant gas* (the platina plates having been made the positive poles of a voltaic pile (570.) in acid), very different results occurred. A mixture was made of 29·2 volumes hydrogen and 14·6 volumes oxygen, being the proportions for water; and to this was added another mixture of 3 volumes oxygen and 1 volume olefiant gas, so that the olefiant gas formed but $\frac{1}{47}$th part of the whole; yet in this mixture the platina plate would not act in forty-five hours. The failure was not for want of any power in the plate, for when after that time it was taken out of this mixture and put into one of oxygen and hydrogen, it immediately acted, and in seven minutes caused explosion of the gas. This result was obtained several times, and when larger proportions of olefiant gas were used the action seemed still more hopeless.

641. A mixture of forty-nine volumes oxygen and hydrogen (638.) with one volume of olefiant gas had a well-prepared platina plate introduced. The diminution of gas was scarcely sensible at the end of two hours, during which it was watched; but on examination twenty-four hours afterwards, the tube was found blown to pieces. The action, therefore, though it had been very much retarded, had occurred at last, and risen to a maximum.

642. With a mixture of ninety-nine volumes of oxygen and hydrogen (638.) with one of olefiant gas, a feeble action was evident at the end of fifty minutes; it went on accelerating (630.) until the eighty-fifth minute, and then became so intense that the

gas exploded. Here also the retarding effect of the olefiant gas was very beautifully illustrated.

643. Plates prepared by alkali and acid (605.) produced corresponding effects.

644. It is perfectly clear from these experiments, that *olefiant gas*, even in small quantities, has a very remarkable influence in preventing the combination of oxygen and hydrogen under these circumstances, and yet without at all injuring or affecting the power of the platina.

645. Another striking illustration of similar interference may be shown in *carbonic oxide*, especially if contrasted with *carbonic acid*. A mixture of one volume oxygen and hydrogen (638.) with four volumes of carbonic acid was affected at once by a platina plate prepared with acid, &c. (605.), and in one hour and a quarter nearly all the oxygen and hydrogen was gone. Mixtures containing less carbonic acid were still more readily affected.

646. But when carbonic oxide was substituted for the carbonic acid, not the slightest effect of combination was produced; and when the carbonic oxide was only one eighth of the whole volume, no action occurred in forty and fifty hours. Yet the plates had not lost their power; for being taken out and put into pure oxygen and hydrogen, they acted well and at once.

647. Two volumes of carbonic oxide and one of oxygen were mingled with nine volumes of oxygen and hydrogen (638.). This mixture was not affected by a plate which had been made positive in acid, though it remained in it fifteen hours. But when to the same volumes of carbonic oxide and oxygen were added thirty-three volumes of oxygen and hydrogen, the carbonic oxide being then only $\frac{1}{8}$th part of the whole, the plate acted, slowly at first, and at the end of forty-two minutes the gases exploded.

648. These experiments were extended to various gases and vapours, the general results of which may be given as follow. Oxygen, hydrogen, nitrogen, and nitrous oxide, when used to dilute the mixture of oxygen and hydrogen, did not prevent the action of the plates even when they made up four fifths of the whole volume of gas acted upon. Nor was the retardation so great in any case as might have been expected from the mere dilution of the oxygen and hydrogen, and the consequent mechanical obstruction to its contact with the platina. The order in which carbonic acid and these substances seemed to stand was as follows, the first interfering least with the action; *nitrous oxide, hydrogen, carbonic acid, nitrogen, oxygen*: but it is possible the plates were not equally well prepared in all, and that other circumstances also were unequal; consequently more numerous experiments would be required to establish the order accurately.

649. As to cases of *retardation*, the powers of olefiant gas and carbonic oxide have been already described. Mixtures of oxygen and hydrogen, containing from $\frac{1}{8}$th to $\frac{1}{80}$th of sulphuretted hydrogen or phosphuretted hydrogen, seemed to show a little action at first, but were not further affected by the prepared plates, though in contact

with them for seventy hours. When the plates were removed they had lost all power over pure oxygen and hydrogen, and the interference of these gases was therefore of a different nature from that of the two former, having permanently affected the plate.

650. A small piece of cork was dipped in sulphuret of carbon and passed up through water into a tube containing oxygen and hydrogen (638.), so as to diffuse a portion of its vapour through the gases. A plate being introduced appeared at first to act a little, but after sixty-one hours the diminution was very small. Upon putting the same plate into a pure mixture of oxygen and hydrogen it acted at once and powerfully, having apparently suffered no diminution of its force.

651. A little vapour of ether being mixed with the oxygen and hydrogen retarded the action of the plate, but did not prevent it altogether. A little of the vapour of the condensed oil-gas liquor* retarded the action still more, but not nearly so much as an equal volume of olefiant gas would have done. In both these cases it was the original oxygen and hydrogen which combined together, the ether and the oil-gas vapour remaining unaffected, and in both cases the plates retained the power of acting on fresh oxygen and hydrogen.

652. Spongy platina was then used in place of the plates, and jets of hydrogen mingled with the different gases thrown against it in air. The results were exactly of the same kind, although presented occasionally in a more imposing form. Thus, mixtures of one volume of olefiant gas or carbonic oxide with three of hydrogen could not heat the spongy platina when the experiments were commenced at common temperatures; but a mixture of equal volumes of nitrogen and hydrogen acted very well, causing ignition. With carbonic acid the results were still stronger. A mixture of three volumes of that gas with one of hydrogen caused ignition of the platina, yet that mixture would not continue to burn from the jet when attempts were made to light it by a taper. A mixture even of *seven* volumes of carbonic acid and *one* of hydrogen will thus cause the ignition of cold spongy platina, and yet, as if to supply a contrast, than which none can be greater, it cannot burn at a taper, but causes the extinction of the latter. On the other hand, the mixtures of carbonic oxide or olefiant gas, which can do nothing with the platina, are inflamed by the taper, burning well.

653. Hydrogen mingled with the vapour of ether or oil-gas liquor causes the ignition of the spongy platina. The mixture with oil-gas burns with a flame far brighter than that of the mixture of hydrogen and olefiant gas already referred to, so that it would appear that the retarding action of the hydro-carbons is not at all in proportion merely to the quantity of carbon present.

654. In connexion with these interferences, I must state that hydrogen itself, prepared from steam passed over ignited iron, was found when mingled with oxygen to resist the action of platina. It had stood over water seven days, and had lost all fetid smell; but a jet of it would not cause the ignition of spongy platina, commencing at common temperatures; nor would it combine with oxygen in a tube either under

* Philosophical Transactions, 1825, p. 440.

the influence of a prepared plate or of spongy platina. A mixture of one volume of this gas with three of pure hydrogen, and the due proportion of oxygen, was not affected by plates after fifty hours. I am inclined to refer the effect to carbonic oxide present in the gas, but have not had time to verify the suspicion. The power of the plates was not destroyed (640. 646.).

655. Such are the general facts of these remarkable interferences. Whether the effect produced by such small quantities of certain gases depends upon any direct action which they may exert upon the particles of oxygen and hydrogen, by which the latter are rendered less inclined to combine, or whether it depends upon their modifying the action of the plate temporarily (for they produce no real change on it), by investing it through the agency of a stronger attraction than that of the hydrogen, or otherwise, remains to be decided by more extended experiments.

656. The theory of action which I have given for the original phenomena appears to me quite sufficient to account for all the effects by reference to known properties, and dispenses with the assumption of any new power of matter. I have pursued this subject at some length, as one of great consequence, because I am convinced that the superficial actions of matter, whether between two bodies, or of one piece of the same body, and the actions of particles not directly or strongly in combination, are becoming daily more and more important to our theories of chemical as well as mechanical philosophy*. In all ordinary cases of combustion it is evident that an action of the kind, considered either upon the surface of the carbon in the fire, or that in the bright part of a flame, must have great influence over the combinations there taking place.

657. The condition of elasticity upon the exterior of the gaseous or vaporous mass already referred to (626. 627.), must be connected directly with the action of solid bodies as nuclei on vapours, causing condensation upon them in preference to any condensation in the vapours themselves; and in the well-known effect of nuclei on solutions a similar condition may have existence (623.), for an analogy in condition exists between the parts of a body in solution, and those of a body in the vaporous or gaseous state. This thought leads us to the consideration of what are the respective conditions at the surfaces of contact of two portions of the same substance at the same temperature, one in the solid or liquid, and the other in the vaporous state; as, for instance, steam and water. It would seem that the particles of vapour next to the particles of liquid are in a different relation to the latter to what they would be with respect to

* As a curious illustration of the influence of mechanical forces over chemical affinity, I will quote the refusal of certain substances to effloresce when their surfaces are perfect, which yield immediately upon the surface being broken. If crystals of carbonate of soda, or phosphate of soda, or sulphate of soda, having no part of their surfaces broken, be preserved from external violence, they will not effloresce. I have thus retained crystals of carbonate of soda perfectly transparent and unchanged from September 1827 to January 1833; and crystals of sulphate of soda from May 1832 to the present time, November 1833. If any part of the surface were scratched or broken, then efflorescence began at that part, and covered the whole. The crystals were merely placed in evaporating basins and covered with paper.

any other liquid or solid substance; as, for instance, mercury or platina, if they were made to replace the water, i. e. if the view of independent action which I have taken (626. 627.) as a consequence of DALTON's principles be correct. It would also seem that the mutual relation of similar particles, and the indifference of dissimilar particles which DALTON has established as a matter of fact amongst gases and vapours, extends to a certain degree amongst solids and fluids, that is, when they are in relation by contact with vapours, either of their own substance or of other bodies. But though I view these points as of great importance with respect to the relations existing between different substances and their physical constitution in the solid, liquid, or gaseous state, I have not sufficiently considered them to venture any strong opinions or statements here.

658. There are numerous well-known cases in which substances, such as oxygen and hydrogen, act readily in their *nascent* state, and produce chemical changes which they are not able to effect if once they have assumed the gaseous condition. Such instances are very common at the poles of the voltaic pile, and are, I think, easily accounted for, if it be considered that at the moment of separation of any such particle it is entirely surrounded by other particles of a *different* kind with which it is in close contact, and has not yet assumed those relations and conditions which it has in its fully developed state, and which it can only assume by association with other particles of its own kind. For, at the moment, its elasticity is absent, and it is in the same relation to particles with which it is in contact, and for which it has an affinity, as the particles of oxygen and hydrogen are to each other on the surface of clean platina (626. 627.).

659. The singular effects of retardation produced by very small quantities of some gases, and not by large quantities of others (640. 645. 652.), if dependent upon any relation of the added gas to the surface of the solid, will then probably be found immediately connected with the curious phenomena which are presented by different gases when passing through narrow tubes at low pressures, which I observed many years ago*; and this action of surfaces must, I think, influence the highly interesting phenomena of the diffusion of gases, at least in the form in which it has been experimented upon by Mr. GRAHAM in 1829 and 1831†, and also by Dr. MITCHELL of Philadelphia‡ in 1830. It seems very probable that if such a substance as spongy platina were used, another law for the diffusion of gases under the circumstances would come out than that obtained by the use of plaster of Paris.

660. I intended to have followed this section by one on the secondary piles of RITTER, and the peculiar properties of the poles of the pile, or of metals through which electricity has passed, which have been observed by RITTER, VAN MARUM, YELIN, DE LA RIVE, MARIANINI, BERZELIUS, and others. It appears to me that all these

* Quarterly Journal of Science, 1819, vol. vii. p. 106.

† Quarterly Journal of Science, vol. xxviii. p. 74., and Edinburgh Transactions, 1831.

‡ Journal of the Royal Institution for 1831, p. 101.

phenomena bear a satisfactory explanation on known principles, connected with the investigation just terminated, and do not require the assumption of any new state or new property. But as the experiments advanced, especially those of MARIANINI, require very careful repetition and examination, the necessity of pursuing the subject of electro-chemical decomposition obliges me for a time to defer the researches to which I have just referred.

Royal Institution,
November 30, 1833.

VI. *Experimental Researches in Electricity.—Seventh Series.* By MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 11. *On Electro-chemical Decomposition, continued.* ¶ iv. *On some general conditions of Electro-decomposition.* ¶ v. *On a new Measurer of Volta-electricity.* ¶ vi. *On the primitive or secondary character of bodies evolved in Electro-decomposition.* ¶ vii. *On the definite nature and extent of Electro-chemical Decompositions.* §. 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

Preliminary.

661. THE theory which I believe to be a true expression of the facts of electro-chemical decomposition, and which I have therefore detailed in a former series of these Researches, is so much at variance with those previously advanced, that I find the greatest difficulty in stating results, as I think, correctly, whilst limited to the use of terms which are current with a certain accepted meaning. Of this kind is the term pole, with its prefixes of positive and negative, and the attached ideas of attraction and repulsion. The general phraseology is that the positive pole *attracts* oxygen, acids, &c., or more cautiously, that it *determines* their evolution upon the surface; and that the negative pole acts in an equal manner upon hydrogen, combustibles, metals, and bases. According to my view, the determining force is *not* at the poles, but *within* the decomposing body; and the oxygen and acids are rendered at the *negative* extremity of that body, whilst hydrogen, metals, &c., are evolved at the *positive* extremity (518. 524.).

662. To avoid, therefore, confusion and circumlocution, and for the sake of greater precision of expression than I can otherwise obtain, I have deliberately considered the subject with two friends, and with their assistance and concurrence in framing them, I purpose henceforward using certain other terms, which I will now define. The poles, as they are usually called, are only the doors or ways by which the electric current passes into and out of the decomposing body (556.); and they of course, when in contact with that body, are the limits of its extent in the direction of the current. The term has been generally applied to the metal surfaces in contact with the decomposing substance; but whether philosophers generally would also apply it to the

surfaces of air (465. 471.) and water (493.), against which I have effected electro-chemical decomposition, is subject to doubt. In place of the term pole, I propose using that of *Electrode**, and I mean thereby that substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current.

663. The surfaces at which, according to the common phraseology, the electric current enters and leaves a decomposing body, are most important places of action, and require to be distinguished apart from the poles, with which they are mostly, and the electrodes, with which they are always, in contact. Wishing for a natural standard of electric direction to which I might refer these, expressive of their difference and at the same time free from all theory, I have thought it might be found in the earth. If the magnetism of the earth be due to electric currents passing round it, the latter must be in a constant direction, which, according to present usage of speech, would be from east to west, or, which will strengthen this help to the memory, that in which the sun appears to move. If in any case of electro-decomposition we consider the decomposing body as placed so that the current passing through it shall be in the same direction, and parallel to that supposed to exist in the earth, then the surfaces at which the electricity is passing into and out of the substance would have an invariable reference, and exhibit constantly the same relations of powers. Upon this notion we purpose calling that towards the east the *anode*†, and that towards the west the *cathode*‡; and whatever changes may take place in our views of the nature of electricity and electrical action, as they must affect the natural standard referred to in the same direction, and to an equal amount with any decomposing substances to which these terms may at any time be applied, there seems no reason to expect that they will lead to confusion, or tend in any way to support false views. The *anode* is therefore that surface at which the electric current, according to our present expression, enters: it is the negative extremity of the decomposing body; is where oxygen, chlorine, acids, &c., are evolved; and is against or opposite the positive electrode. The *cathode* is that surface at which the current leaves the decomposing body, and is its positive extremity; the combustible bodies, metals, alkalies, and bases, are evolved there, and it is in contact with the negative electrode.

664. I shall have occasion in these Researches, also, to class bodies together according to certain relations derived from their electrical actions (822.); and wishing to express those relations without at the same time involving the expression of any hypothetical views, I intend using the following names and terms. Many bodies are decomposed directly by the electric current, their elements being set free; these I propose to call *electrolytes*§. Water, therefore, is an electrolyte. The bodies which,

* ἤλεκτρον, and ὁδος *a way*.

† ἀνα *upwards*, ὁδος *a way*; the way which the sun rises.

‡ κατα *downwards*, ὁδος *a way*; the way which the sun sets.

§ ἤλεκτρον, and λυω *solvo*. N. Electrolyte, V. Electrolyze.

like nitric or sulphuric acids, are decomposed in a secondary manner (752. 757.), are not included under this term. Then for *electro-chemically decomposed*, I shall often use the term *electrolyzed*, derived in the same way, and implying that the body spoken of is separated into its components under the influence of electricity: it is analogous in its sense and sound to *analyze*, which is derived in a similar manner. The term *electrolytical* will be understood at once. Muriatic acid is electrolytical, boracic acid is not.

665. Finally, I require a term to express those bodies which can pass to the *electrodes*, or, as they are usually called, the poles. Substances are frequently spoken of as being *electro-negative*, or *electro-positive*, according as they go under the supposed influence of a direct attraction to the positive or negative pole. But these terms are much too significant for the use to which I should have to put them; for though the meanings are perhaps right, they are only hypothetical, and may be wrong; and then, through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual views of those engaged in pursuing it. I propose to distinguish these bodies by calling those *anions** which go to the *anode* of the decomposing body; and those passing to the *cathode*, *cations*†; and when I have occasion to speak of these together, I shall call them *ions*. Thus, the chloride of lead is an *electrolyte*, and when *electrolyzed* evolves the two *ions*, chlorine and lead, the former being an *anion*, and the latter a *cation*.

666. These terms being once well defined, will, I hope, in their use enable me to avoid much periphrasis and ambiguity of expression. I do not mean to press them into service more frequently than will be required, for I am fully aware that names are one thing and science another‡.

667. It will be well understood that I am giving no opinion respecting the nature of the electric current now, beyond what I have done on a former occasion (283. 517.); and that though I speak of the current as proceeding from the parts which are positive to those which are negative (663.), it is merely in accordance with the conventional, though in some degree tacit, agreement entered into by scientific men, that they may have a constant, certain, and definite means of referring to the direction of the forces of that current.

¶ iv. *On some general conditions of Electro-chemical Decomposition.*

669. From the period when electro-chemical decomposition was first effected to the present time, it has been a remark, that those elements which, in the ordinary phenomena of chemical affinity, were the most directly opposed to each other, and combined with the greatest attractive force, were those which were the most readily evolved at the opposite extremities of the decomposing bodies (549.).

* ἀνιον *that which goes up.* (Neuter participle.) † κατιον *that which goes down.*

‡ Since this paper was read, I have changed some of the terms which were first proposed, that I might employ only such as were at the same time simple in their nature, clear in their reference, and free from hypothesis.

670. If this result was evident when water was supposed to be essential to, and was present, in almost every case of such decomposition (472.), it is far more evident now that it has been shown and proved that water is not necessarily concerned in the phenomena (474.), and that other bodies much surpass it in some of the effects supposed to be peculiar to that substance.

671. Water, from its constitution and the nature of its elements, and from its frequent presence in cases of electrolytic action, has hitherto stood foremost in this respect. Though a compound formed by very powerful affinity, it yields up its elements under the influence of a very feeble electric current; and it is doubtful whether a case of electrolyzation can occur, where, being present, it is not resolved into its first principles.

672. The various oxides, chlorides, iodides, and salts (402.), which I have shown are decomposable by the electric current when in the liquid state, under the same general law with water, illustrate in an equally striking manner the activity, in such decompositions, of elements directly and powerfully opposed to each other by their chemical relations.

673. On the other hand, bodies dependent on weak affinities very rarely give way. Take, for instance, glasses: many of those formed of silica, lime, alkali, and oxide of lead, may be considered as little more than solutions of substances one in another*. If bottle-glass be fused, and subjected to the voltaic pile, it does not appear to be at all decomposed (408.). If flint-glass, which contains substances more directly opposed, be operated upon, it suffers some decomposition; and if borate of lead glass, which is a definite chemical compound, be experimented with, it readily yields up its elements (408).

674. But the result which is found to be so striking in the instances quoted is not at all borne out by reference to other cases where a similar consequence might have been expected. It may be said, that my own theory of electro-chemical decomposition would lead to the expectation that all compound bodies should give way under the influence of the electric current with a facility proportionate to the strength of the affinity by which their elements, either proximate or ultimate, are combined. I am not sure that that follows as a consequence of the theory; but if the objection be supposed one presented by facts, I have no doubt it will be removed when we obtain a more intimate acquaintance with, and precise idea of, the nature of chemical affinity and the mode of action of an electric current over it (518. 524.): besides which, it is just as directly opposed to any other theory of electro-chemical decomposition as the one I have propounded; for if it be admitted, as is generally the case, that the more directly bodies are opposed to each other in their attractive forces, the more powerfully do they combine, then the objection applies with equal force to any of the theories of electrolyzation which have been considered, and is an addition to those which I have taken against them.

* Philosophical Transactions, 1830, p. 49.

675. Amongst powerful compounds which are not decomposed, boracic acid stands prominent (408.). Then again, the iodide of sulphur, and the chlorides of sulphur, phosphorus, and carbon, are not decomposable under common circumstances, though their elements are of a nature which would lead to a contrary expectation. Chloride of antimony (402. 690.), the hydro-carbons, acetic acid, ammonia, and many other bodies undecomposable by the voltaic pile, would seem to be formed by an affinity sufficiently strong to indicate that the elements were so far contrasted in their nature as to sanction the expectation that the pile would separate them, especially as in some cases of mere solution (530. 544.), where the affinity must by comparison be very weak, separation takes place*.

676. It must not be forgotten, however, that much of this difficulty, and perhaps the whole, may depend upon the absence of conducting power, which, preventing the transmission of the current, prevents of course the effects due to it. All known compounds being non-conductors when solid, but conductors when liquid, are decomposed, with *perhaps* the single exception at present known of periodide of mercury (679. 691.); and even water itself, which so easily yields up its elements when the current passes, if rendered quite pure, scarcely suffers change, because it then becomes a very bad conductor.

677. If it should hereafter be proved that the want of decomposition in those cases where, from chemical considerations, it might be so strongly expected (669. 674. 672.), is due to the absence or deficiency of conducting power, it would also be proved, at the same time, that decomposition *depends* upon conduction, and not the latter upon the former (413.); and in water this seems to be very nearly decided. On the other hand, the conclusion is almost irresistible, that in electrolytes the power of transmitting the electricity across the substance is dependent upon their capability of suffering decomposition; taking place only whilst they are decomposing, and being proportionate to the quantity of elements separated (821.). I may not, however, stop to discuss this point experimentally at present.

678. When a compound contains such elements as are known to pass towards the opposite extremities of the voltaic pile, still the proportions in which they are present appear to be intimately connected with capability in the compound of suffering or resisting decomposition. Thus, the protochloride of tin readily conducts, and is decomposed (402.), but the perchloride neither conducts nor is decomposed (406.). The protiodide of tin is decomposed when fluid (402.); the periodide is not (405.). The periodide of mercury when fused is not decomposed (691.), even though it does conduct. I was unable to contrast it with the protiodide, the latter being converted into mercury and periodide by heat.

679. These important differences induced me to look more closely to certain binary compounds, with a view of ascertaining whether a *law* regulating the *decomposability*

* With regard to solution, I have met with some reasons for supposing that it will probably disappear as a cause of transference, and intend resuming the consideration at a convenient opportunity.

according to some *relation of the proportionals or equivalents* of the elements, could be discovered. The proto compounds only, amongst those just referred to, were decomposable; and on referring to the substances quoted to illustrate the force and generality of the law of conduction and decomposition which I discovered (402.), it will be found that all the oxides, chlorides, and iodides subject to it, except the chloride of antimony and the periodide of mercury, (to which may now perhaps be added corrosive sublimate,) are also decomposable, whilst many per compounds of the same elements, not subject to the law, were not so (405. 406.).

680. The substances which appeared to form the strongest exceptions to this gegeral result were such bodies as the sulphuric, phosphoric, nitric, arsenic, and other acids.

681. On experimenting with sulphuric acid, I found no reason to believe that it was by itself a conductor of, or decomposable by, electricity, although I had previously been of that opinion (552.). When very strong it is a much worse conductor than if diluted*. If then subjected to the action of a powerful battery, oxygen appears at the *anode*, or positive electrode, although much is absorbed (728.), and hydrogen and sulphur appear at the *cathode*, or negative electrode. Now the hydrogen has with me always been pure, not sulphuretted, and has been deficient in proportion to the sulphur present, so that it is evident that when decomposition occurred water must have been decomposed. I endeavoured to make the experiment with anhydrous sulphuric acid. It appeared to me that in that state, when fused, sulphuric acid was not a conductor, nor decomposed; but I had not enough of the dry acid in my possession to allow me to decide the point satisfactorily. My belief is, that when sulphur appears by the action of the pile on sulphuric acid, it is the result of a secondary action, and that the acid itself is not electrolyzable (757.).

682. Phosphoric acid is, I believe, also in the same condition; but I have found it impossible to decide the point, because of the difficulty of operating on fused anhydrous phosphoric acid. Phosphoric acid which has once obtained water cannot be deprived of it by heat alone. When heated, the hydrated acid volatilizes. Upon subjecting phosphoric acid, fused upon the ring end of a wire (401.), to the action of the voltaic apparatus, it conducted, and was decomposed; but gas, which I believe to be hydrogen, was always evolved at the negative electrode, and the wire was not affected as would have happened had phosphorus been separated. Gas was also evolved at the positive electrode. From all the facts, I conclude it was the water and not the acid which was decomposed.

683. *Arsenic acid.* This substance conducted, and was decomposed; but it contained water, and I was unable at the time to press the investigation so as to ascertain whether a fusible anhydrous arsenic acid could be obtained. It forms, therefore, at present no exception to the general result.

684. Nitrous acid, obtained by distilling nitrate of lead, and keeping it in contact

* De la Rive.

with strong sulphuric acid, was found to conduct and decompose slowly. But on examination there were strong reasons for believing that water was present, and that the decomposition and conduction depended upon it. I endeavoured to prepare a perfectly anhydrous portion, but could not spare the time required to procure an unexceptionable result.

685. Nitric acid is a substance which I believe is not decomposed directly by the electric current. As I want the facts in illustration of the distinction existing between primary and secondary decomposition, I will merely refer to them in this place (752.).

686. That these mineral acids should confer facility of conduction and decomposition on water, is no proof that they are competent to favour and suffer these actions in themselves. Boracic acid does the same thing, though not decomposable. M. De la Rive has pointed out that chlorine has this power also; but being to us an elementary substance, it cannot be due to its capability of suffering decomposition.

687. *Chloride of sulphur* does not conduct, nor is it decomposed. It consists of single proportionals of its elements, but is not on that account an exception to the rule (679.), which does not affirm that *all* compounds of single proportionals of elements are decomposable, but that such as are decomposable are so constituted.

688. *Protochloride of phosphorus* does not conduct nor become decomposed.

689. *Protochloride of carbon* does not conduct nor suffer decomposition. In association with this substance, I submitted the *hydro-chloride of carbon* from olefiant gas and chlorine to the action of the electric current; but it also refused to conduct or yield up its elements.

690. With regard to the exceptions (679.), upon closer examination, some of them disappear. Chloride of antimony (a compound of one proportional of antimony and one and a half of chlorine) of recent preparation was put into a tube (fig. 13.) (789.), and submitted when fused to the action of the current, the positive electrode being of plumbago. No electricity passed, and no appearance of decomposition was visible at first; but when the positive and negative electrodes were brought very near each other in the chloride, then a feeble action occurred and a feeble current passed. The effect altogether was so small (although quite amenable to the law before given), and so unlike the decomposition and conduction occurring in all the other cases, that I attribute it to the presence of a minute quantity of water, (for which this and many other chlorides have strong attractions, producing hydrated chlorides,) or perhaps of a true protochloride consisting of single proportionals (695. 796.).

691. *Periodide of mercury* being examined in the same manner, was found most distinctly to insulate whilst solid, but conduct when fluid, according to the law of *liquido-conduction* (402.); but there was no appearance of decomposition. No iodine appeared at the *anode*, nor mercury or other substance at the *cathode*. The case is, therefore, no exception to the rule, that only compounds of single proportionals are decomposable; but it is an exception, and I think the only one, to the statement, that

all bodies subject to the law of liquido-conduction are decomposable. I incline, however, to believe, that a portion of protiodide of mercury is retained dissolved in the periodide, and that to its slow decomposition the feeble conducting power is due. Periodide would be formed, as a secondary result, at the *anode*; and the mercury at the *cathode* would also form, as a secondary result, protiodide. Both these bodies would mingle with the fluid mass, and thus no final separation appear, notwithstanding the continued decomposition.

692. When *perchloride of mercury* was subjected to the voltaic current, it did not conduct in the solid state, but it did conduct when fluid. I think, also, that in the latter case it was decomposed; but there are many interfering circumstances which require examination before a positive conclusion can be drawn.

693. When the ordinary protoxide of antimony is subjected to the voltaic current in a fused state, it also is decomposed, although the effect from other causes soon ceases (402. 802.). This oxide consists of one proportional of antimony and one and a half of oxygen, and is therefore an exception to the general law assumed. But in working with this oxide and the chloride, I observed facts which lead me to doubt whether the compounds usually called the protoxide and the protochloride do not often contain other compounds, consisting of single proportions, which are the true proto compounds, and which, in the case of the oxide, might give rise to the decomposition above described.

694. The ordinary sulphuret of antimony is considered as being the compound with the smallest quantity of sulphur, and analogous in its proportions to the ordinary protoxide. But I find that if it be fused with metallic antimony, a new sulphuret is formed, containing much more of the metal than the former, and separating distinctly, when fused, both from the pure metal on the one hand, and the ordinary grey sulphuret on the other. In some rough experiments, the metal thus taken up by the ordinary sulphuret of antimony was equal to half the proportion of that previously in the sulphuret, in which case the new sulphuret would consist of *single* proportionals.

695. When this new sulphuret was dissolved in muriatic acid, although a little antimony separated, yet it appeared to me that a true protochloride, consisting of *single* proportionals, was formed, and from that, by alkalies, &c., a true protoxide, consisting also of *single* proportionals was obtainable. But I could not stop to ascertain this matter strictly by analysis.

696. I believe, however, that there is such an oxide; that it is often present in variable proportions in what is commonly called protoxide, throwing uncertainty upon the results of its analysis, and causing the electrolytic decomposition above described.

697. Upon the whole, it appears probable that all those binary compounds of elementary bodies which are capable of being electrolyzed when fluid, but not whilst solid, according to the law of liquido-conduction (394.), consist of single proportionals of their elementary principles; and it may be because of their departure from this

simplicity of composition, that boracic acid, ammonia, perchlorides, periodides, and many other direct compounds of elements, are indecomposable.

698. With regard to salts and combinations of compound bodies, the same simple relation does not appear to hold good. I could not decide this by bisulphates of the alkalies, for as long as the second proportion of acid remained, water was retained with it. The fused salt, therefore, conducted, and was decomposed; but hydrogen always appeared at the negative electrode.

699. A biphosphate of soda was prepared by heating, and ultimately fusing, the ammonia-phosphate of soda. In this case the fused bisalt conducted, and was decomposed; but a little gas appeared at the negative electrode, and though I believe the salt itself was electrolyzed, I am not quite satisfied that water was entirely absent.

700. Then a biborate of soda was prepared; and this, I think, is an unobjectionable case. The salt, when fused, conducted, and was decomposed, and gas appeared at both electrodes: even when the boracic acid was increased to three proportionals the same effect took place.

701. Hence this class of compound combinations does not seem to be subject to the same simple law as the former class of binary combinations. Whether we may find reason to consider them as mere solutions of the compound of single proportionals in the excess of acid, is a matter which, with some apparent exceptions occurring amongst the sulphurets, must be left for decision by future examination.

702. In any investigation of these points, great care must be taken to exclude water; for if present, secondary effects are so frequently produced as often seemingly to indicate an electro-decomposition of substances, when no true result of the kind has occurred (742. &c.).

703. It is evident that all the cases in which decomposition *does not occur may* depend upon the want of conduction (677. 413.); but that does not at all lessen the interest excited by seeing the great difference of effect due to a change, not in the nature of the elements, but merely in their proportions, especially in any attempt which may be made to elucidate and expound the beautiful theory put forth by Sir Humphry Davy*, and illustrated by Berzelius and other eminent philosophers, that ordinary chemical affinity is a mere result of the electrical attractions of the particles of matter.

¶ v. *On a new Measurer of Volta-electricity.*

704. I have already said, when engaged in reducing common and voltaic electricity to one standard of measurement (377.), and again when introducing my theory of electro-chemical decomposition (504. 505. 510.), that the chemical decomposing action of a current *is constant for a constant quantity of electricity*, notwithstanding the greatest variations in its sources, in its intensity, in the size of the *electrodes* used, in the nature of the conductors (or non-conductors (307.)) through which it is

* Philosophical Transactions, 1807, pp. 32, 39; also 1826, pp. 387, 389.

passed, or in other circumstances. The conclusive proofs of the truth of these statements shall be given almost immediately (783. &c.).

705. I endeavoured upon this law to construct an instrument which should measure out the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a *comparative standard* of effect, or as a *positive measurer* of this subtile agent.

706. There is no substance better fitted, under ordinary circumstances, to be the indicating body in such an instrument than water; for it is decomposed with facility when rendered a better conductor by the addition of acids or salts; its elements may in numerous cases be obtained and collected without any embarrassment from secon dary action, and, being gaseous, they are in the best physical condition for separation and measurement. Water, therefore, acidulated by sulphuric acid, is the substance I shall generally refer to, although it may become expedient in peculiar cases or forms of experiment to use other bodies (843.).

707. The first precaution needful in the construction of the instrument was to avoid the recombination of the evolved gases, an effect which the positive electrode has been found so capable of producing (571.). For this purpose various forms of decomposing apparatus were used. The first consisted of straight tubes, each containing a plate and wire of platina soldered together by gold, and fixed hermetically in the glass at the closed extremity of the tube (Plate I. fig. 5.). The tubes were about eight inches long, 0·7 of an inch in diameter, and graduated. The platina plates were about an inch long, as wide as the tubes would permit, and adjusted as near to the mouths of the tubes as was consistent with the safe collection of the gases evolved. In certain cases, where it was required to evolve the elements upon as small a surface as possible, the metallic extremity, instead of being a plate, consisted of the wire bent into the form of a ring (fig. 6.). When these tubes were used as measurers, they were filled with the dilute sulphuric acid, and inverted in a basin of the same liquid (fig. 7.), being placed in an inclined position, with their mouths near to each other, that as little decomposing matter should intervene as possible; and also, in such a direction that the platina plates should be in vertical planes (720.).

708. Another form of apparatus was that delineated (fig. 8.). The tube is bent in the middle; one end is closed; in that end is fixed a wire and plate, *a*, proceeding so far downwards, that, when in the position figured, it shall be as near to the angle as possible, consistently with the collection, at the closed extremity of the tube, of all the gas evolved against it. The plane of this plate is also perpendicular (720.). The other metallic termination, *b*, is introduced at the time decomposition is to be effected, being brought as near the angle as possible, without causing any gas to pass from it towards the closed end of the instrument. The gas evolved against it is allowed to escape.

709. The third form of apparatus contains both electrodes in the same tube; the transmission, therefore, of the electricity, and the consequent decomposition, is far

more rapid than in the separate tubes. The resulting gas is the sum of the portions evolved at the two electrodes, and the instrument is better adapted than either of the former as a measurer of the quantity of voltaic electricity transmitted in ordinary cases. It consists of a straight tube (fig. 9.) closed at the upper extremity, and graduated, through the sides of which pass the platina wires (being fused into the glass), which are connected with two plates within. The tube is fitted by grinding into one mouth of a double-necked bottle. If the latter be one half or two thirds full of the dilute sulphuric acid, it will, upon inclination of the whole, flow into the tube and fill it. When an electric current is passed through the instrument, the gases evolved against the plates collect in the upper portion of the tube, and are not subject to the recombining power of the platina.

710. Another form of the instrument is given at fig. 10.

711. A fifth form is delineated (fig. 11.). This I have found exceedingly useful in experiments continued in succession for days together, and where large quantities of indicating gas were to be collected. It is fixed on a weighted foot, and has the form of a small retort containing the two electrodes: the neck is narrow, and sufficiently long to deliver gas issuing from it into a jar placed in a small pneumatic trough. The electrode chamber, sealed hermetically at the part held in the stand, is five inches in length, and 0·6 of an inch in diameter; the neck about nine inches in length, and 0·4 of an inch in diameter internally. The figure will fully indicate the construction.

712. It can hardly be requisite to remark, that in the arrangement of any of these forms of apparatus, they, and the wires connecting them with the substance, which is collaterally subjected to the action of the same electric current, should be so far insulated as to ensure a certainty that all the electricity which passes through the one shall also be transmitted through the other.

713. Next to the precaution of collecting the gases, if mingled, out of contact with the platinum, was the necessity of testing the law of a *definite electrolytic* action, upon water at least, under all varieties of condition; that, with a conviction of its certainty, might also be obtained a knowledge of those interfering circumstances which would require to be practically guarded against.

714. The first point investigated was the influence or indifference of extensive variations in the size of the electrodes, for which purpose instruments like those last described (709. 710. 711.) were used. One of these had plates 0·7 of an inch wide, and nearly four inches long; another had plates only 0·5 of an inch wide, and 0·8 of an inch long; a third had wires 0·02 of an inch in diameter, and three inches long; and a fourth similar wires only half an inch in length. Yet when these were filled with dilute sulphuric acid, and, being placed in succession, had one common current of electricity passed through them, very nearly the same quantity of gas was evolved in all. The difference was sometimes in favour of one, and sometimes on the side of another; but the general result was that the largest quantity of gases was evolved upon the smaller surface of the wires.

715. Experiments of a similar kind were made with the single-plate, straight tubes (707.), and also with the curved tubes (708.), with similar consequences; and when these, with the former tubes, were arranged together in various ways, the result, as to the equality of action of large and small metallic surfaces when delivering and receiving the same current of electricity, was constantly the same. As an illustration, the following numbers are given. An instrument with two wires evolved 74·3 volumes of mixed gases; another with plates 73·25 volumes; whilst the sum of the oxygen and hydrogen in two separate tubes amounted to 73·65 volumes. In an other experiment the volumes were 55·3, 55·3, and 54·4.

716. But it was observed in these experiments, that in single-plate tubes (707.) more hydrogen was evolved at the negative electrode than was proportionate to the oxygen at the positive electrode; and generally, also, more than was proportionate to the oxygen and hydrogen in a double-plate tube. Upon more minutely examining these effects, I was led to refer them, and also the differences between wires and plates (714.), to the solubility of the gases evolved, especially at the positive electrode.

717. When the positive and negative electrodes are equal in surface, the bubbles which rise from them in dilute sulphuric acid are always different in character. Those from the positive plate are exceedingly small, and separate instantly from every part of the surface of the metal, in consequence of its perfect cleanliness (633.); whilst in the liquid they give it a hazy appearance, from their number and minuteness; are easily carried down by currents; and therefore not only present far greater surface of contact with the liquid than larger bubbles would do, but are retained a much longer time in mixture with it. But the bubbles at the negative surface, though they constitute twice the volume of the gas at the positive electrode, are nevertheless very inferior in number. They do not rise so universally from every part of the surface, but seem to be evolved at different points; and though so much larger, they appear to cling to the metal, separating with difficulty from it, and when separated, instantly rising to the top of the liquid. If, therefore, oxygen and hydrogen had equal solubility in, or powers of combining with, water under similar circumstances, still under the present conditions the oxygen would be far the most liable to solution; but when to these is added its well known power of forming a compound with water, it is no longer surprising that such a compound should be produced in small quantities at the positive electrode; and indeed the bleaching power which some philosophers have observed in a solution at this electrode, when chlorine and similar bodies have been carefully excluded, is probably due to the formation there, in this manner, of oxywater.

718. That more gas was collected from the wires than from the plates, I attribute to the circumstance, that as equal quantities were evolved in equal times, the bubbles at the wires having been more rapidly produced, in relation to any part of the surface, must have been much larger; have been therefore in contact with the fluid by a much

smaller surface, and for a much shorter time than those at the plates; hence less solution and a greater collection.

719. There was also another effect produced, especially by the use of large electrodes, which was both a consequence and a proof of the solution of part of the gas evolved there. The collected gas, when examined, was found to contain small portions of nitrogen. This I attribute to the presence of air dissolved in the acid used for decomposition. It is a well-known fact, that when bubbles of a gas but slightly soluble in water or solutions pass through them, the portion of this gas which is dissolved displaces a portion of that previously in union with the liquid: and so, in the decompositions under consideration, as the oxygen dissolves, it displaces a part of the air, or at least of the nitrogen, previously united to the acid; and this proceeds *most extensively* with large plates, because the gas evolved at them is in the most favourable condition for solution.

720. With the intention of avoiding this solubility of the gases as much as possible, I arranged the decomposing plates in a vertical position (707. 708.), that the bubbles might quickly escape upwards, and that the downward currents in the fluid should not meet ascending currents of gas. This precaution I found to assist greatly in producing constant results, and especially in experiments to be hereafter referred to, in which other liquids than dilute sulphuric acid, as for instance solution of potash, were used.

721. The irregularities in the indications of the measurer proposed, arising from the solubility just referred to, are but small, and may be very nearly corrected by comparing the results of two or three experiments. They may also be almost entirely avoided by selecting that solution which is found to favour them in the least degree (728.); and still further by collecting the hydrogen only, and using that as the indicating gas; for being much less soluble than oxygen, being evolved with twice the rapidity and in larger bubbles (717.), it can be collected more perfectly and in greater purity.

722. From the foregoing and many other experiments, it results that *variation in the size of the electrodes causes no variation in the chemical action of a given quantity of electricity upon water.*

723. The next point in regard to which the principle of constant electro-chemical action was tested, was *variation of intensity.* In the first place, the preceding experiments were repeated, using batteries of an *equal* number of plates, *strongly* and *weakly* charged; but the results were alike. They were then repeated, using batteries sometimes containing forty, and at other times only five pairs of plates; but the results were still the same. *Variations therefore in the intensity,* caused by difference in the strength of charge, or in the number of alternations used, *produced no difference as to the equal action of large and small electrodes.*

724. Still these results did not prove that variation in the intensity of the current was not accompanied by a corresponding variation in the electro-chemical effects,

since the actions at *all* the surfaces might have increased or diminished together. The deficiency in the evidence is, however, completely supplied by the former experiments on different-sized electrodes; for with variation in the size of these, a variation in the intensity must have occurred. The intensity of an electric current traversing conductors alike in their nature, quality, and length, is probably as the quantity of electricity passing through a given sectional area perpendicular to the current, divided by the time (360. *note*); and therefore when large plates were contrasted with wires separated by an equal length of the same decomposing conductor (714.), whilst one current of electricity passed through both arrangements, that electricity must have been in a very different state, as to *tension*, between the plates and between the wires; yet the chemical results were the same.

725. The difference in intensity, under the circumstances described, may be easily shown practically, by arranging two decomposing apparatus as in fig. 12, where the same fluid is subjected to the decomposing power of the same current of electricity, passing in the vessel A. between large platina plates, and in the vessel B. between small wires. If a third decomposing apparatus, such as that delineated fig. 11. (711.), be connected with the wires at *a b*, fig. 12, it will serve sufficiently well, by the degree of decomposition occurring in it, to indicate the relative state of the two plates as to intensity; and if it then be applied in the same way, as a test of the state of the wires at *a' b'*, it will, by the increase of decomposition within, show how much greater the intensity is there than at the former points. The connexions of P and N with the voltaic battery are of course to be continued during the whole time.

726. A third form of experiment in which difference of intensity was obtained, for the purpose of testing the principle of equal chemical action, was to arrange three volta-electrometers, so that after the electric current had passed through one, it should divide into two parts, which, after traversing each one of the remaining instruments, should reunite. The sum of the decomposition in the two latter vessels was always equal to the decomposition in the former vessel. But the *intensity* of the divided current could not be the same as that it had in its original state; and therefore *variation of intensity has no influence on the results if the quantity of electricity remain the same.* The experiment, in fact, resolves itself simply into an increase in the size of the electrodes (725.).

727. The *third point*, in respect to which the principle of equal electro-chemical action on water was tested, was *variation of the strength of the solution used.* In order to render the water a conductor, sulphuric acid had been added to it (707.); and it did not seem unlikely that this substance, with many others, might render the water more subject to decomposition, the electricity remaining the same in quantity. But such did not prove to be the case. Diluted sulphuric acid, of different strengths, was introduced into different decomposing apparatus, and submitted simultaneously to the action of the same electric current (714.). Slight differences occurred, as before, sometimes in one direction, sometimes in another; but the final result was, that

exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity, though the sulphuric acid in some was seventyfold what it was in others. The strengths used were of specific gravity 1·495, and downwards.

728. When an acid having a specific gravity of about 1·336 was employed, the results were most uniform, and the oxygen and hydrogen (716.) most constantly in the right proportion to each other. Such an acid gave more gas than one much weaker acted upon by the same current, apparently because it had less solvent power. If the acid were very strong, then a remarkable disappearance of oxygen took place; thus, one made by mixing two measures of strong oil of vitriol with one of water, gave forty-two volumes of hydrogen, but only twelve of oxygen. The hydrogen was very nearly the same with that evolved from acid of the specific gravity 1·232. I have not yet had time to examine minutely the circumstances attending the disappearance of the oxygen in this case, but imagine it is due to the formation of oxywater, which THÉNARD has shown is favoured by the presence of acid.

729. Although not necessary for the practical use of the instrument I am describing, yet as connected with the important point of constant electro-chemical action upon water, I now investigated the effects produced by an electric current passing through aqueous solutions of acids, salts, and compounds, exceedingly different from each other in their nature, and found them to yield astonishingly uniform results. But many of them which are connected with a secondary action will be more usefully described hereafter (778.).

730. When solutions of caustic potassa or soda, or sulphate of magnesia, or sulphate of soda, were acted upon by the electric current, just as much oxygen and hydrogen was evolved from them as from the diluted sulphuric acid, with which they were compared. When a solution of ammonia, rendered a better conductor by sulphate of ammonia (554.), or a solution of subcarbonate of potassa was experimented with, the *hydrogen* evolved was in the same quantity as that set free from the diluted sulphuric acid with which they were compared. Hence *changes in the nature of the solution do not alter the constancy of electrolytic action upon water*.

731. I have already said, respecting large and small electrodes, that change of order caused no change in the general effect (715.). The same was the case with different solutions, or with different intensities; and however the circumstances of an experiment might be varied, the results came forth exceedingly consistent, and proved that the electro-chemical action was still the same.

732. I consider the foregoing investigation as sufficient to prove the very extraordinary and important principle with respect to WATER, *that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportionate to the quantity of electricity which has passed*, notwithstanding the thousand variations in the conditions and circumstances under which it may at the time be placed; and further, that when the interference of certain secondary effects (742. &c.), together with the solution or recombination of the gas and the evolution of air, are guarded against,

the products of the decomposition may be collected with such accuracy, as to afford a very excellent and valuable measurer of the electricity concerned in their evolution.

733. The forms of instrument which I have given, figg. 9, 10, 11. (709. 710. 711.), are probably those which will be found most useful, as they indicate the quantity of electricity by the largest volume of gases, and cause the least obstruction to the passage of the current. The fluid which my present experience leads me to prefer, is a solution of sulphuric acid of specific gravity about 1·336, or from that to specific gravity 1·25; but it is very essential that there should be no organic substance, nor any vegetable acid, nor other body, which, by being liable to the action of the oxygen or hydrogen evolved at the electrodes (773. &c.), shall diminish their quantity, or add other gases to them.

734. In many cases when the instrument is used as a *comparative standard*, or even as *a measurer*, it may be desirable to collect the hydrogen only, as being less liable to absorption or disappearance in other ways than the oxygen; whilst at the same time its volume is so large, as to render it a good and sensible indicator. In such cases the first and second form of apparatus have been used, figg. 7, 8. (707. 708.). The indications obtained were very constant, the variations being much smaller than in those forms of apparatus collecting both gases; and they can also be procured when solutions are used in comparative experiments, which, yielding no oxygen or only secondary results of its action, can give no indications if the educts at both electrodes be collected. Such is the case when solutions of ammonia, muriatic acid, chlorides, iodides, acetates, or other vegetable salts, &c., are employed.

735. In a few cases, as where solutions of metallic salts liable to reduction at the negative electrode are acted upon, the oxygen may be advantageously used as the measuring substance. This is the case, for instance, with sulphate of copper.

736. There are therefore two general forms of the instrument which I submit as a measurer of electricity. One, in which both the gases of the water decomposed are collected (709. 710. 711.); and the other, in which a single gas, as the hydrogen only, is used (707. 708.). When referred to as a *comparative instrument*, (a use I shall now make of it very extensively,) it will not often require particular precaution in the observation; but when used as an *absolute measurer*, it will be needful that the barometric pressure and the temperature be taken into account, and that the graduation of the instruments should be to one scale; the hundredths and smaller divisions of a cubical inch are quite fit for this purpose, and the hundredth may be very conveniently taken as indicating a DEGREE of electricity.

737. It can scarcely be needful to point out further than has been done how this instrument is to be used. It is to be introduced into the course of the electric current, the action of which is to be exerted anywhere else, and if 60° or 70° of electricity are to be measured out, either in one or several portions, the current, whether strong or weak, is to be continued until the gas in the tube occupies that number of divisions or hundredths of a cubical inch. Or if a quantity competent to produce a certain

effect is to be measured, the effect is to be obtained, and then the indication read off. In exact experiments it is necessary to correct the volume of gas for changes in temperature and pressure, and especially for moisture*. For the latter object the volta-electrometer (fig. 11.) is most accurate, as its gas can be measured over water, whilst the others retain it over acid or saline solutions.

738. I have not hesitated to apply the term *degree*, in analogy with the use made of it with respect to another most important imponderable agent, namely, heat; and as the definite expansion of air, water, mercury, &c., is there made use of to measure heat, so the equally definite evolution of gases is here turned to a similar use for electricity.

739. The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. For without being at all affected by variations in time or intensity, or alterations in the current itself, of any kind, or from any cause, or even of intermissions of action, it takes note with accuracy of the quantity of electricity which has passed through it, and reveals that quantity by inspection; I have therefore named it a VOLTA-ELECTROMETER.

740. Another mode of measuring volta-electricity may be adopted with advantage in many cases, dependent on the quantities of metals or other substances evolved either as primary or as secondary results; but I refrain from enlarging on this use of the products, until the principles on which their constancy depends have been fully established (791. 843.).

741. By the aid of this instrument I have been able to establish the definite character of electro-chemical action in its most general sense; and I am persuaded it will become of the utmost use in the extensions of the science which these views afford. I do not pretend to have made its detail perfect, but to have demonstrated the truth of the principle, and the utility of the application.

¶ vi. *On the primary or secondary character of the bodies evolved at the Electrodes.*

742. Before the *volta-electrometer* could be employed in determining, as a *general law*, the constancy of electro-decomposition, it became necessary to examine a distinction, already recognised among scientific men, relative to the products of that action, namely, their primitive or secondary character; and, if possible, by some general rule or principle, to decide when they were of the one or the other kind. It will appear hereafter that great mistakes respecting electro-chemical action and its consequences, have arisen from confounding these two classes of results together.

743. When a substance under decomposition yields at the electrodes those bodies uncombined and unaltered which the electric current has separated, then they may be considered as primary results, even though themselves compounds. Thus the oxygen and hydrogen from water are primary results; and so also are the acid and alkali (themselves compound bodies) evolved from sulphate of soda. But when the sub-

* For a simple table of correction for moisture, I may take the liberty of referring to my Chemical Manipulation, edition of 1830, p. 376.

stances separated by the current are changed at the electrodes before their appearance, then they give rise to secondary results, although in many cases the bodies evolved are elementary.

744. These secondary results occur in two ways, being sometimes due to the mutual action of the evolving substance and the matter of the electrode, and sometimes to its action upon the substances contained in the decomposing conductor itself. Thus, when carbon is made the positive electrode in dilute sulphuric acid, carbonic oxide and carbonic acid appear there instead of oxygen; for the latter, acting upon the matter of the electrode, produces these secondary results. Or if the positive electrode, in a solution of nitrate or acetate of lead, be platina, then peroxide of lead appears there, equally a secondary result with the former, but now depending upon an action of the oxygen on a substance in the solution. Again, when ammonia is decomposed by platina electrodes, nitrogen appears at the *anode**; but though an *elementary* body, it is a *secondary* result in this case, being derived from the chemical action of the oxygen electrically evolved there, upon the ammonia in the surrounding solution (554.). In the same manner when aqueous solutions of metallic salts are decomposed by the current, the metals evolved at the *cathode*, though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

745. Many of these secondary results are extremely valuable; for instance, all the interesting compounds which M. Becquerel has obtained by feeble electric currents are of this nature; but they are essentially chemical, and must, in the theory of electrolytic action, be carefully distinguished from those which are directly due to the action of the electric current.

746. The nature of the substances evolved will often lead to a correct judgement of their primary or secondary character, but is not sufficient alone to establish that point. Thus, nitrogen is said to be attracted sometimes by the positive and sometimes by the negative electrode, according to the bodies with which it may be combined (554. 555.), and it is on such occasions evidently viewed as a primary result†; but I think I shall show, that, when it appears at the positive electrode, or rather at the *anode*, it is a secondary result (748.). Thus, also, Sir Humphry Davy‡, and with him the great body of chemical philosophers, (including myself,) have given the appearance of copper, lead, tin, silver, gold, &c., at the negative electrode, when their aqueous solutions were acted upon by the voltaic current, as proofs that the metals, as a class, were attracted to that surface; thus assuming the metal in each case to be a primary result. These however, I expect to prove, are all secondary results; the mere consequence of chemical action, and no proofs of the attraction or the law announced §.

* Annales de Chimie, 1804, tom. li. p. 167. † Ibid. tom. li. p. 172.

‡ Elements of Chemical Philosophy, pp. 144. 161.

§ It is remarkable that up to 1804 it was the received opinion that the metals were reduced by the nascent hydrogen. At that date the general opinion was reversed by Hisinger and Berzelius (Annales de Chimie,

747. But when we take to our assistance the law of *constant electro-chemical action* already proved with regard to water (732.), and which I hope to extend satisfactorily to all bodies (821.), and consider the *quantities* as well as the *nature* of the substances set free, a generally accurate judgement of the primary or secondary character of the results may be formed: and this important point, so essential to the theory of electro-decomposition, since it decides what are the particles directly under the influence of the current, (distinguishing them from such as are not affected,) and what are the results to be expected, may be established with such degree of certainty as to remove innumerable ambiguities and doubtful considerations from this branch of the science.

748. Let us apply these principles to the case of ammonia, and the supposed determination of nitrogen to one or the other *electrode* (554. 555.). A pure strong solution of ammonia is as bad a conductor, and therefore as little liable to electro-decomposition, as pure water; but when sulphate of ammonia is dissolved in it, the whole becomes a conductor; nitrogen *almost* and occasionally *quite* pure is evolved at the *anode*, and hydrogen at the *cathode*; the ratio of the volume of the former to that of the latter varying, but being as 1 to about 3 or 4. This result would seem at first to imply that the electric current had decomposed ammonia, and that the nitrogen had been determined towards the positive electrode. But when the electricity used was measured out by the volta-electrometer (707. 736.), it was found that the hydrogen obtained was exactly in the proportion which would have been supplied by decomposed water, whilst the nitrogen had no certain or constant relation whatever. When, upon multiplying experiments, it was found that, by using a stronger or weaker solution, or a more or less powerful battery, the gas evolved at the *anode* was a mixture of oxygen and nitrogen, varying both in proportion and absolute quantity, whilst the hydrogen at the *cathode* remained constant, no doubt could be entertained that the nitrogen at the *anode* was a secondary result, depending upon the chemical action of the nascent oxygen, determined to that surface by the electric current, upon the ammonia in solution. It was the water, therefore, which was electrolyzed, not the ammonia. Further, the experiment gives no real indication of the tendency of the element nitrogen to either one electrode or the other; nor do I know of any experiment with nitric acid, or other compounds of nitrogen, which shows the tendency of this element, under the influence of the electric current, to pass in either direction along its course.

749. As another illustration of secondary results, the effects on a solution of acetate of potassa may be quoted. When a very strong solution was used, more gas was evolved at the *anode* than at the *cathode*, in the proportion of 4 to 3 nearly: that from the *anode* was a mixture of carbonic oxide and carbonic acid; that from the *cathode* pure hydrogen. When a much weaker solution was used, less gas was evolved at the *anode* than at the *cathode*; and it now contained carburetted hydrogen, as well as carbonic oxide and car-

1804, tom. li. p. 174.), who stated that the metals were evolved directly by the electricity: in which opinion it appears, from that time, Davy coincided (Philosophical Transactions, 1826, p. 388.).

bonic acid. This result of carburetted hydrogen at the positive electrode has a very anomalous appearance, if considered as an immediate consequence of the decomposing power of the current. It, however, as well as the carbonic oxide and acid, is only a *secondary result*; for it is the water alone which suffers electro-decomposition, and it is the oxygen eliminated at the *anode* which, reacting on the acetic acid, in the midst of which it is evolved, produces those substances that finally appear there. This is fully proved by experiments with the volta-electrometer (707.); for then the hydrogen evolved from the acetate at the *cathode* is always found to be definite, being exactly proportionate to the electricity which has passed through the solution, and, in quantity, the same as the hydrogen evolved in the volta-electrometer itself. The appearance of the carbon in combination with the hydrogen at the positive electrode, and its non-appearance at the negative electrode, are in curious contrast with the results which might have been expected from the law usually accepted respecting the final places of the elements.

750. If the salt in solution be an acetate of lead, then the results at both electrodes are secondary, and cannot be used to estimate or express the amount of electro-chemical action, except by a circuitous process (843.). In place of oxygen, or even the gases already described (749.), peroxide of lead now appears at the positive, and lead itself at the negative electrode. When other metallic solutions are used, containing, for instance, peroxides, as that of copper, combined with this or any other decomposable acid, still more complicated results will be obtained; which, viewed as direct results of the electro-chemical action, will, in their proportions, present nothing but confusion, but will appear perfectly harmonious and simple if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.

751. I have experimented upon many bodies, with a view to determine whether the results were primary or secondary. I have been surprised to find how many of them, in ordinary cases, are of the latter class, and how frequently water is the only body electrolyzed in instances where other substances have been supposed to give way. Some of these results I will give in as few words as possible.

752. *Nitric acid.*—When very strong, it conducted well, and yielded oxygen at the positive electrode. No gas appeared at the negative electrode; but nitrous acid, and apparently nitric oxide, were formed there, which, dissolving, rendered the acid yellow or red, and at last even effervescent, from the spontaneous separation of nitric oxide. Upon diluting the acid with its bulk or more of water, gas appeared at the negative electrode. Its quantity could be varied by variations, either in the strength of the acid or of the voltaic current: for that acid from which no gas separated at the *cathode*, with a weak voltaic battery, did evolve gas there with a stronger; and that battery which evolved no gas there, with a strong acid, did cause its evolution with an acid more dilute. The gas at the *anode* was always oxygen; that at the *cathode* hydrogen. When the quantity of products was examined by the volta-electro-

meter (707.), the oxygen, whether from strong or weak acid, proved to be in the same proportion as from water. When the acid was diluted to specific gravity 1·24, or less, the hydrogen also proved to be the same in quantity as from water. Hence I conclude that the nitric acid does not undergo electro-chemical decomposition, but the water only; that the oxygen at the *anode* is always a primary result, but that the products at the *cathode* are often secondary, and due to the reaction of the hydrogen upon the nitric acid.

753. *Nitre.*—A solution of this salt yields very variable results, according as one or other form of tube is used, or as the electrodes are large or small. Sometimes the whole of the hydrogen of the water decomposed may be obtained at the negative electrode; at other times, only a part of it, because of the ready formation of secondary results. The solution is a very excellent conductor of electricity.

754. *Nitrate of ammonia*, in aqueous solution, gives rise to secondary results very varied and uncertain in their proportions.

755. *Sulphurous acid.*—Pure liquid sulphurous acid does not conduct nor suffer decomposition by the voltaic current*, but, when dissolved in water, the solution acquires conducting power, and is decomposed, yielding oxygen at the *anode*, and hydrogen and sulphur at the *cathode*.

756. A solution containing sulphuric acid in addition, was a better conductor. It gave very little gas at either electrode: that at the *anode* was oxygen, that at the *cathode* pure hydrogen. From the *cathode* also rose a white turbid stream, consisting of diffused sulphur, which soon rendered the whole solution milky. The volumes of gases were in no regular proportion to the quantities evolved from water in the volta-electrometer. I conclude that the sulphurous acid was not at all affected by the electric current in any of these cases, and that the water present was the only body electro-chemically decomposed; that, at the *anode*, the oxygen from the water converted the sulphurous acid into sulphuric acid, and, at the *cathode*, the hydrogen electrically evolved decomposed the sulphurous acid, combining with its oxygen, and setting its sulphur free. I conclude that the sulphur at the negative electrode was only a secondary result; and, in fact, no part of it was found combined with the small portion of hydrogen which escaped when weak solutions of sulphurous acid were used.

757. *Sulphuric acid.*—I have already given my reasons for concluding that sulphuric acid is not electrolyzable, i. e. not decomposable directly by the electric current, but occasionally suffering by a secondary action at the *cathode* from the hydrogen evolved there (681.). In the year 1800, Davy considered the sulphur from sulphuric acid as the result of the action of the nascent hydrogen †. In 1804, Hisinger and Berzelius stated that it was the direct result of the action of the voltaic pile ‡; an opinion which from that time Davy seems to have adopted, and which has since been

* See also De la Rive, Bibliothèque Universelle, tom. xl. p. 205; or Quarterly Journal of Science, vol. xxvii. p. 407.

† Nicholson's Quarterly Journal, vol. iv. pp. 280, 281.

‡ Annales de Chimie, 1804, tom. li. p. 173.

commonly received by all. The change of my own opinion requires that I should correct what I have already said of the decomposition of sulphuric acid in a former series of these Researches (552.): I do not now think that the appearance of the sulphur at the negative electrode is an immediate consequence of electrolytic action.

758. *Muriatic acid.*—A strong solution gave hydrogen at the negative electrode, and chlorine only at the positive electrode; of the latter, a part acted on the platina and a part was dissolved. A minute bubble of gas remained; it was not oxygen, but probably air previously held in solution.

759. It was an important matter to determine whether the chlorine was a primary result, or only a secondary product, due to the action of the oxygen evolved from water at the *anode* upon the muriatic acid; i. e. whether the muriatic acid was electrolyzable, and if so, whether the decomposition was *definite*.

760. The muriatic acid was gradually diluted. One part with six of water gave only chlorine at the *anode*. One part with eight of water gave only chlorine; with nine of water, a little oxygen appeared with the chlorine: but the occurrence or non-occurrence of oxygen at these strengths depended, in part, on the strength of the voltaic battery used. With fifteen parts of water, a little oxygen, with much chlorine, was evolved at the *anode*. As the solution was now becoming a bad conductor of electricity, sulphuric acid was added to it: this caused more ready decomposition, but did not sensibly alter the proportion of chlorine and oxygen.

761. The muriatic acid was now diluted with 100 times its volume of dilute sulphuric acid. It still gave a large proportion of chlorine at the *anode*, mingled with oxygen; and the result was the same, whether a voltaic battery of 40 pairs of plates or one containing only 5 pairs were used. With acid of this strength, the oxygen evolved at the *anode* was to the hydrogen at the *cathode*, in volume, as 17 is to 64; and therefore the chlorine would have been 30 volumes, had it not been dissolved by the fluid.

762. Next, with respect to the quantity of elements evolved. On using the volta-electrometer, it was found that, whether the strongest or the weakest muriatic acid were used, whether chlorine alone or chlorine mingled with oxygen appeared at the *anode*, still the hydrogen evolved at the *cathode* was a constant quantity, i. e. exactly the *same* as the hydrogen which the *same quantity of electricity* could evolve from water.

763. This constancy does not decide whether the muriatic acid is electrolyzed or not, although it proves that if so, it must be in definite proportions to the quantity of electricity used. Other considerations may, however, be allowed to decide the point. The analogy between chlorine and oxygen, in their relations to hydrogen, is so strong, as to lead almost to the certainty, that, when combined with that element, they would perform similar parts in the process of electro-decomposition. They both unite with it in single proportional or equivalent quantities; and, the number of proportionals appearing to have an intimate and important relation to the decomposability of a

body (697.), those in muriatic acid, as well as in water, are the most favourable, or those, perhaps even necessary, to decomposition. In other binary compounds of chlorine also, where nothing equivocal depending on the simultaneous presence of it and oxygen is involved, the chlorine is directly eliminated at the *anode* by the electric current. Such is the case with the chloride of lead (395.), which may be justly compared with protoxide of lead (402.), and stands in the same relation to it as muriatic acid to water. The chlorides of potassium, sodium, barium, &c., are in the same relation to the protoxides of the same metals, and present the same results under the influence of the electric current (402.).

764. From all the experiments, combined with these considerations, I conclude that muriatic acid is decomposed by the direct influence of the electric current, and that the quantities evolved are, and therefore the chemical action is, *definite for a definite quantity of electricity.* For though I have not collected and measured the chlorine, in its separate state, at the *anode*, there can exist no doubt as to its being proportional to the hydrogen at the *cathode*; and the results are therefore sufficient to establish the general law of *constant electro-chemical action* in the case of muriatic acid.

765. In the dilute acid (761.), I conclude that a part of the water is electro-chemically decomposed, giving origin to the oxygen, which appears mingled with the chlorine at the *anode*. The oxygen *may* be viewed as a secondary result; but I incline to believe that it is not so: for, if it were, it might be expected in largest proportion from the stronger acid, whereas the reverse is the fact. This consideration, with others, also leads me to conclude that muriatic acid is more easily decomposed by the electric current than water; since, even when diluted with eight or nine times its quantity of the latter fluid, it alone gives way, the water remaining unaffected.

766. *Chlorides.*—On using solutions of chlorides in water,—for instance, the chlorides of sodium or calcium,—there was evolution of chlorine only at the positive electrode, and of hydrogen, with the oxide of the base, as soda or lime, at the negative electrode. The process of decomposition may be viewed as proceeding in two or three ways, all terminating in the same results. Perhaps the simplest is to consider the chloride as the substance electrolyzed, its chlorine being determined to and evolved at the *anode*, and its metal passing to the *cathode*, where, finding no more chlorine, it acts upon the water, producing hydrogen and an oxide as secondary results. As the discussion would detain me from more important matter, and is not of immediate consequence, I shall defer it for the present. It is, however, of *great consequence* to state, that, on using the volta-electrometer, the hydrogen in both cases was definite; and if the results do not prove the definite decomposition of chlorides, (which shall be proved elsewhere,—789. 794. 814.,) they are not in the slightest degree opposed to such a conclusion, and *do* support the *general law*.

767. *Hydriodic acid.*—A solution of hydriodic acid was affected exactly in the same manner as muriatic acid. When strong, hydrogen was evolved at the negative electrode, in definite proportion to the quantity of electricity which had passed, i. e. in

the same proportion as was evolved by the same current from water; and iodine without any oxygen was evolved at the positive electrode. But when diluted, small quantities of oxygen appeared with the iodine at the *anode*, the proportion of hydrogen at the *cathode* remaining undisturbed.

768. I believe the decomposition of the hydriodic acid in this case to be direct, for the reasons already given respecting muriatic acid (763. 764.).

769. *Iodides.*—A solution of iodide of potassium being subjected to the voltaic current, iodine appeared at the positive electrode (without any oxygen), and hydrogen with free alkali at the negative electrode. The same observations as to the mode of decomposition are applicable here as were made in relation to the chlorides when in solution (766.).

770. *Hydro-fluoric acid and fluorides.*—Solution of hydro-fluoric acid did not appear to be decomposed under the influence of the electric current: it was the water which gave way apparently. The fused fluorides were electrolyzed (417.); but having during these actions obtained *fluorine* in the separate state, I think it better to refer to a future series of these Researches, in which I purpose giving a fuller account of the results than would be consistent with propriety here.

771. *Hydro-cyanic acid* in solution conducts very badly. The definite proportion of hydrogen (equal to that from water) was set free at the *cathode*, whilst at the *anode* a small quantity of oxygen was evolved and apparently a solution of cyanogen formed. The action altogether corresponded with that on a dilute muriatic or hydriodic acid. When the hydro-cyanic acid was made a better conductor by sulphuric acid, the same results occurred.

Cyanides.—With a solution of the cyanide of potassium, the result was precisely the same as with a chloride or iodide. No oxygen was evolved at the positive electrode, but a brown solution formed there. For the reasons given when speaking of the chlorides (766.), and because a fused cyanide of potassium evolves cyanogen at the positive electrode*, I incline to believe that the cyanide in solution is *directly* decomposed.

772. *Ferro-cyanic acid* and the *ferro-cyanides*, as also *sulpho-cyanic acid* and the *sulpho-cyanides*, presented results corresponding with those just described (771.).

773. *Acetic acid.* Glacial acetic acid, when fused (405.), is not decomposed by, nor does it conduct, electricity. On adding a little water to it, still there were no signs of action; on adding more water, it acted slowly and about as water alone would do. Dilute sulphuric acid was added to it in order to make it a better conductor; then the definite proportion of hydrogen was evolved at the *cathode*, and a mixture of oxygen in very deficient quantity, with carbonic acid, and a little carbonic oxide, at the *anode*. Hence it appears that acetic acid is not electrolyzable, but that a portion of it is decomposed by the oxygen evolved at the *anode*, producing secondary results,

* It is a very remarkable thing to see carbon and nitrogen in this case determined powerfully towards the positive surface of the voltaic battery; but it is perfectly in harmony with the theory of electro-chemical decomposition which I have advanced.

varying with the strength of the acid, the intensity of the current, and other circumstances.

774. *Acetates.*—One of these has been referred to already, as affording only secondary results relative to the acetic acid (749.). With many of the metallic acetates the results at both electrodes are secondary (746. 750.).

Acetate of soda fused and anhydrous is directly decomposed, being, as I believe, a true electrolyte, and evolving soda and acetic acid at the *cathode* and *anode*. These, however, have no sensible duration, but are immediately resolved into other substances; charcoal, sodiuretted hydrogen, &c., being set free at the former, and as far as I could judge under the circumstances, acetic acid mingled with carbonic oxide, carbonic acid, &c., at the latter.

775. *Tartaric acid.*—Pure solution of tartaric acid is almost as bad a conductor as pure water. On adding sulphuric acid to it, it conducted well, the results at the positive electrode being primary or secondary in different proportions, according to variations in the strength of the acid and the power of the electric current (752.). Alkaline tartrates gave a large proportion of secondary results at the positive electrode. The hydrogen at the negative electrode remained constant unless certain metallic salts were used.

776. Solutions of salts containing other vegetable acids, as the benzoates; of sugar, gum, &c., dissolved in dilute sulphuric acid; of resin, albumen, &c., dissolved in alkalies, were in turn submitted to the electrolytic power of the voltaic current. In all these cases, secondary results to a greater or smaller extent were produced at the positive electrode.

777. In concluding this division of these Researches, it cannot but occur to the mind that the final result of the action of the electric current upon substances placed between the electrodes, instead of being simple may be very complicated. There are two modes by which these substances may be decomposed, either by the direct force of the electric current, or by the action of bodies which that current may evolve. There are also two modes by which new compounds may be formed, i. e. by combination of the evolving substances whilst in their nascent state (658.), directly with the matter of the electrode; or else their combination with those bodies, which being contained in, or associated with, the decomposing conductor, are necessarily present at the *anode* and *cathode*. The complexity is rendered still greater by the circumstance that two or more of these actions may occur simultaneously, and also in variable proportions to each other. But it may in a great measure be resolved by attention to the principles already laid down (747.).

778. When *aqueous* solutions of bodies are used, secondary results are exceedingly frequent. Even when the water is not present in large quantity, but is merely that of combination, still secondary results often ensue: for instance, it is very possible that in Sir Humphry Davy's decomposition of the hydrates of potassa and soda, a part of the potassium produced was the result of a secondary action. Hence, also, a frequent

cause for the disappearance of the oxygen and hydrogen which would otherwise be evolved: and when hydrogen does *not* appear at the *cathode* in an *aqueous solution*, it perhaps always indicates that a secondary action has taken place there. No exception to this rule has as yet occurred to my observation.

779. Secondary actions are *not confined to aqueous solutions*, or cases where water is present. For instance, various chlorides acted upon, when fused (402.), by platina electrodes, have the chlorine determined electrically to the *anode*. In many cases, as with the chlorides of lead, potassium, barium, &c., the chlorine acts on the platina and forms a compound with it, which dissolves; but when protochloride of tin is used, the chlorine at the *anode* does not act upon the platina, but upon the chloride already there, forming a perchloride which rises in vapour (790. 804.). These are, therefore, instances of secondary actions of both kinds, produced in bodies containing no water.

780. The production of boron from fused borax (402. 417.) is also a case of secondary action; for boracic acid is not decomposable by electricity (408.), and it was the sodium evolved at the *cathode* which, reacting on the boracic acid around it, took oxygen from it and set boron free in the experiments formerly described.

781. Secondary actions have already, in the hands of M. Becquerel, produced many interesting results in the formation of compounds; some of them new, others imitations of those occurring naturally*. It is probable they may prove equally interesting in an opposite direction, i. e. as affording cases of analytic decomposition. Much information regarding the composition, and perhaps even the arrangement of the particles of such bodies as the vegetable acids and alkalies, and organic compounds generally, will probably be obtained by submitting them to the action of nascent oxygen, hydrogen, chlorine, &c., at the electrodes; and the action seems the more promising, because of the thorough command which we possess over attendant circumstances, such as the strength of the current, the size of the electrodes, the nature of the decomposing conductor, its strength, &c., all of which may be expected to have their corresponding influence upon the final result.

782. It is to me a great satisfaction that the extreme variety of secondary results have presented nothing opposed to the doctrine of a constant and definite electro-chemical action, to the particular consideration of which I shall now proceed.

¶ vii. *On the definite nature and extent of Electro-chemical Decomposition.*

783. In the third series of these Researches, after proving the identity of electricities derived from different sources, and showing, by actual measurement, the extraordinary quantity of electricity evolved by a very feeble voltaic arrangement (371. 376.), I announced a law, derived from experiment, which seemed to me of the utmost importance to the science of electricity in general, and that branch of it denominated electro-chemistry in particular. The law was expressed thus: *The chemical power of*

* Annales de Chimie, tom. xxxv. p. 113.

a current of electricity is in direct proportion to the absolute quantity of electricity which passes (377.).

784. In the further progress of the successive investigations, I have had frequent occasion to refer to the same law, occasionally in circumstances offering powerful corroboration of its truth (456. 504. 505.); and the present series already supplies numerous new cases in which it holds good (704. 722. 726. 732.). It is now my object to consider this great principle more closely, and to develope some of the consequences to which it leads. That the evidence for it may be the more distinct and applicable, I shall quote cases of decomposition subject to as few interferences from secondary results as possible, effected upon bodies very simple, yet very definite in their nature.

785. In the first place, I consider the law as so fully established with respect to the decomposition of *water*, and under so many circumstances which might be supposed, if anything could, to exert an influence over it, that I may be excused entering into further detail respecting that substance, or even summing up the results here (732). I refer, therefore, to the whole of the subdivision of this series of Researches which contains the account of the *volta-electrometer*.

786. In the next place, I also consider the law as established with respect to *muriatic acid* by the experiments and reasoning already advanced, when speaking of that substance, in the subdivision respecting primary and secondary results (758, &c.).

787. I consider the law as established also with regard to *hydriodic acid* by the experiments and considerations already advanced in the preceding division of this series of Researches (767. 768.).

788. Without speaking with the same confidence, yet from the experiments described, and many others not described, relating to hydro-fluoric, hydro-cyanic, ferrocyanic, and sulpho-cyanic acids (770. 771. 772.), and from the close analogy which holds between these bodies and the hydro-acids of chlorine, iodine, bromine, &c., I consider these also as coming under subjection to the law, and assisting to prove its truth.

789. In the preceding cases, except the first, the water is believed to be inactive; but to avoid any ambiguity arising from its presence, I sought for substances from which it should be absent altogether; and, taking advantage of the law of conduction already developed (380. &c.), soon found abundance, amongst which *protochloride of tin* was first subjected to decomposition in the following manner. A piece of platina wire had one extremity coiled up into a small knob, and having been carefully weighed, was sealed hermetically into a piece of bottle-glass tube, so that the knob should be at the bottom of the tube within (fig. 13.). The tube was suspended by a piece of platina wire, so that the heat of a spirit-lamp could be applied to it. Recently fused protochloride of tin was introduced in sufficient quantity to occupy, when melted, about one half of the tube; the wire of the tube was connected with a volta-electrometer (711.), which was itself connected with the negative end of a voltaic battery; and a platina wire connected with the positive end of the same battery was dipped into the

fused chloride in the tube; being, however, so bent, that it could not by any shake of the hand or apparatus touch the negative electrode at the bottom of the vessel. The whole arrangement is delineated fig. 14.

790. Under these circumstances the chloride of tin was decomposed: the chlorine evolved at the positive electrode formed bichloride of tin (779.), which passed away in fumes, and the tin evolved at the negative electrode combined with the platina, forming an alloy, fusible at the temperature to which the tube was subjected, and therefore never occasioning metallic communication entirely through the decomposing chloride. When the experiment had been continued so long as to yield a reasonable quantity of gas in the volta-electrometer, the battery connexion was broken, the positive electrode removed, and the tube and remaining chloride allowed to cool. When cold, the tube was broken open, the rest of the chloride and the glass being easily separable from the platina wire and its button of alloy. The latter when washed was then reweighed, and the increase gave the weight of the tin reduced.

791. I will give the particular results of one experiment, in illustration of the mode adopted in this and others, the results of which I shall have occasion to quote. The negative electrode weighed at first 20 grains; after the experiment it, with its button of alloy, weighed 23·2 grains. The tin evolved by the electric current at the *cathode* weighed, therefore, 3·2 grains. The quantity of oxygen and hydrogen collected in the volta-electrometer = 3·85 cubic inches. As 100 cubic inches of oxygen and hydrogen, in the proportions to form water, may be considered as weighing 12·92 grains, the 3·85 cubic inches would weigh 0·49742 of a grain; that being, therefore, the weight of water decomposed by the same electric current as was able to decompose such weight of protochloride of tin as could yield 3·2 grains of metal. Now 0·49742 : 3·2 :: 9 the equivalent of water is to 57·9, which should therefore be the equivalent of tin, if the experiment had been made without error, and if the electro-chemical decomposition *is in this case also definite*. In some chemical works 58 is given as the chemical equivalent of tin, in others 57·9. Both are so near to the result of the experiment, and the experiment itself is so subject to slight causes of variation (as from the absorption of gas in the volta-electrometer (716.), &c.), that the numbers leave little doubt of the applicability of the *law of definite action* in this and all similar cases of electro-decomposition.

792. It is not often I have obtained an accordance in numbers so near as that I have just quoted. Four experiments were made on the protochloride of tin, the quantities of gas evolved in the volta-electrometer being from 2·05 to 10·29 cubic inches. The average of the four experiments gave 58·53 as the electro-chemical equivalent for tin.

793. The chloride remaining after the experiment, was pure protochloride of tin; and no one can doubt for a moment that the equivalent of chlorine had been evolved at the *anode*, and having formed bichloride of tin as a secondary result, had passed away.

794. *Chloride of lead* was experimented upon in a manner exactly similar, except that a change was made in the nature of the positive electrode; for as the chlorine evolved at the *anode* forms no perchloride of lead, but acts directly upon the platina, if that metal be used, it produces a solution of chloride of platina in the chloride of lead; in consequence of which a portion of platina can pass to the *cathode*, and will produce a vitiated result. I therefore sought for, and found in plumbago, another substance, which could be used safely as the positive electrode in such bodies as chlorides, iodides, &c. The chlorine or iodine does not act upon it, but is evolved in the free state; and the plumbago has no reaction, under the circumstances, upon the fused chloride or iodide in which it is plunged. Even if a few particles of plumbago should separate by the heat or the mechanical action of the evolved gas, they can do no harm in the chloride.

795. The mean of three experiments gave the number of 100·85 as the equivalent for lead. The chemical equivalent is 103·5. The deficiency in my experiments I attribute to the solution of part of the gas (716.) in the volta-electrometer; but the results leave no doubt on my mind that both the lead and the chlorine are, in this case, evolved in *definite quantities* by the action of a given quantity of electricity (814. &c.).

796. *Chloride of antimony.*—It was in endeavouring to obtain the electro-chemical equivalent of antimony from the chloride that I found reasons for the statement I have made respecting the presence of water in it in an earlier part of these Researches (690. 693. &c.).

797. I endeavoured to experiment upon the *oxide of lead* obtained by fusion and ignition of the nitrate in a platina crucible, but found great difficulty, from the high temperature required for perfect fusion, and the powerful fluxing qualities of the substance. Green glass tubes repeatedly failed. I at last fused the oxide in a small porcelain crucible, heated fully in a charcoal fire; and as it was essential that the evolution of the lead at the *cathode* should take place beneath the surface, the negative electrode was guarded by a green glass tube, fused around it in such a manner as to expose only the knob of platina at the lower end (fig. 15.), so that it could be plunged beneath the surface, and thus exclude contact of air or oxygen with the lead reduced there. A platina wire was employed for the positive electrode, that metal not being subject to any action from the oxygen evolved against it. The arrangement is given fig. 16.

798. In an experiment of this kind the equivalent for the lead came out 93·17, which is very much too small. This, I believe, was because of the small interval between the positive and negative electrodes in the oxide of lead, so that it was not unlikely that some of the froth and bubbles formed by the oxygen at the *anode* should occasionally even touch the lead reduced at the *cathode*, and re-oxidize it. When I endeavoured to correct this by having more litharge, the greater heat required to keep it all fluid caused a quicker action on the crucible, which was soon eaten through, and the experiment stopped.

799. In one experiment of this kind I used borate of lead (408. 673.). It evolves lead, under the influence of the electric current, at the *anode*, and oxygen at the *cathode*; and as the boracic acid is not either directly (408.) or incidentally decomposed during the operation, I expected a result dependent on the oxide of lead. The borate is not so violent a flux as the oxide, but it requires a higher temperature to make it quite liquid; and if not very hot, the bubbles of oxygen cling to the positive electrode, and retard the transfer of electricity. The number for lead came out 101·29, which is so near to 103·5 as to show that the action of the current had been definite.

800. *Oxide of bismuth.*—I found this substance required too high a temperature, and acted too powerfully as a flux, to allow of any experiment being made on it, without the application of more time and care than I could give at present.

801. The ordinary *protoxide of antimony*, which consists of one proportional of metal and one and a half of oxygen, was subjected to the action of the electric current in a green glass tube (789.), surrounded by a jacket of platina foil, and heated in a charcoal fire. The decomposition began and proceeded very well at first, apparently indicating, according to the general law (679. 697.), that this substance was one containing such elements and in such proportions as made it amenable to the power of the electric current. This effect I have already given reasons for supposing may be due to the presence of a true protoxide, consisting of single proportionals (696. 693.). The action soon diminished, and finally ceased, because of the formation of a higher oxide of the metal at the positive electrode. This compound, which was probably the peroxide, being infusible and insoluble in the protoxide, formed a crystalline crust around the positive electrode; and thus insulating it, prevented the transmission of the electricity. Whether if it had been fusible and still immiscible it would have decomposed, is doubtful, because of its departure from the required composition (697.). It was a very natural secondary product at the positive electrode (779.). On opening the tube it was found that a little antimony had been separated at the negative electrode; but the quantity was too small to allow of any quantitative result being obtained.

802. *Iodide of lead.*—This substance can be experimented with in tubes heated by a spirit-lamp (789.); but I obtained no good results from it, whether I used positive electrodes of platina or plumbago. In two experiments the numbers for the lead came out only 75·46 and 73·45, instead of 103·5. This I attribute to the formation of a periodide at the positive electrode, which dissolving in the mass of liquid iodide, came in contact with the lead evolved at the negative electrode, and dissolved part of it, becoming itself again protiodide. Such a periodide does exist; and it is very rarely that the iodide of lead formed by precipitation, and well washed, can be fused without evolving much iodine, from the presence of this percompound; nor does crystallization from its hot aqueous solution free it from this substance. Even when a little of the protiodide and iodine are merely rubbed together in a mortar, a portion of the periodide is formed. And though it is decomposed by being fused and heated

to dull redness for a few minutes, and the whole reduced to protiodide, yet that is not at all opposed to the possibility, that a little of that which is formed in great excess of iodine at the *anode*, should be carried by the rapid currents in the liquid into contact with the *cathode*.

803. This view of the results was strengthened by a third experiment, where the space between the electrodes was increased to one third of an inch; for now the interfering effects were much diminished, and the number of the lead came out 89·04; and it was fully confirmed by the results obtained in the cases of transfer to be immediately described (818.).

The experiments on iodide of lead, therefore, offer no exception to the *general law* under consideration, but, on the contrary, may, from general considerations, be admitted as included in it.

804. *Protiodide of tin.*—This substance, when fused (402.), conducts and is decomposed by the electric current, tin is evolved at the *anode*, and periodide of tin as a secondary result (779. 790.) at the *cathode*. The temperature required for its fusion is too high to allow of the production of any results fit for weighing.

805. *Iodide of potassium* was subjected to electrolytic action in a tube, fig. 13. (789.). The negative electrode was a globule of lead, and I hoped in this way to retain the potassium, and obtain results that could be weighed and compared with the volta-electrometer indication; but the difficulties dependent upon the high temperature required, the action upon the glass, the fusibility of the platina induced by the presence of the lead, and other circumstances, prevented me from obtaining such results. The iodide was decomposed with the evolution of iodine at the *anode*, and of potassium at the *cathode*, as in former cases.

806. In some of these experiments several substances were placed in succession, and decomposed simultaneously by the same electric current: thus, protochloride of tin, chloride of lead, and water, were thus acted on at once. It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen evolved being definite in quantity and electro-chemical equivalents to each other.

807. Let us turn to another kind of proof of the *definite chemical action of electricity*. If any circumstances could be supposed to exert an influence over the quantity of the matters evolved during electrolytic action, one would expect them to be present when electrodes of different substances, and possessing very different chemical affinities for the evolving bodies, were used. Platina has no power in dilute sulphuric acid of combining with the oxygen at the *anode*, though the latter be evolved in the nascent state against it. Copper, on the other hand, immediately unites to the oxygen, as the electric current sets it free from the hydrogen; and zinc is not only able to combine with it, but can, without any help from the electricity, abstract it directly from the water, at the same time setting torrents of hydrogen free. Yet in cases where these three substances were used as the positive electrodes in three similar portions of the same dilute sulphuric acid, specific gravity 1·336, precisely the same quantity of water

was decomposed by the electric current, and precisely the same quantity of hydrogen set free at the *cathodes* of the three solutions.

808. The experiment was made thus. Portions of the dilute sulphuric acid were put into three basins. Three volta-electrometer tubes, of the form figg. 5, 7. were filled with the same acid, and one inverted in each basin (707.). A zinc plate, connected with the positive end of a voltaic battery, was dipped into the first basin, forming the positive electrode there, the hydrogen, which was abundantly evolved from it by the direct action of the acid, being allowed to escape. A copper plate, which dipped into the acid of the second basin, was connected with the negative electrode of the *first* basin; and a platina plate, which dipped into the acid of the third basin, was connected with the negative electrode of the *second* basin. The negative electrode of the third basin was connected with a volta-electrometer (711.), and that with the negative end of the voltaic battery.

809. Immediately that the circuit was complete, the *electro-chemical action* commenced in all the vessels. The hydrogen still rose in, apparently, undiminished quantities from the positive zinc electrode in the first basin. No oxygen was evolved at the positive copper electrode in the second basin, but a sulphate of copper was formed there; whilst in the third basin the positive platina electrode evolved pure oxygen gas, and was itself unaffected. But in *all* the basins the hydrogen liberated at the *negative* platina electrodes was the *same in quantity*, and the same with the volume of hydrogen evolved in the volta-electrometer, showing that in all the vessels the current had decomposed an equal quantity of water. In this trying case, therefore, the *chemical action of electricity* proved to be *perfectly definite*.

810. A similar experiment was made with muriatic acid diluted with its bulk of water. The three positive electrodes were zinc, silver, and platina; the first being able to separate and combine with the chlorine *without* the aid of the current; the second combining with the chlorine only after the current had set it free; and the third rejecting almost the whole of it. The three negative electrodes were, as before, platina plates fixed within glass tubes. In this experiment, as in the former, the quantity of hydrogen evolved at the *cathodes* was the same for all, and the same as the hydrogen evolved in the volta-electrometer. I have already given my reasons for believing that in these experiments it is the muriatic acid which is directly decomposed by the electricity (764.); and the results prove that the quantities so decomposed are *perfectly definite* and proportionate to the quantity of electricity which has passed.

811. In this experiment the chloride of silver formed in the second basin retarded the passage of the current of electricity, by virtue of the law of conduction before described (394.), so that it had to be cleaned off four or five times during the course of the experiment; but this caused no difference between the results of that vessel and the others.

812. Charcoal was used as the positive electrode in both sulphuric and muriatic acids (808. 810.); but this change produced no variation of the results. A zinc positive

electrode, in sulphate of soda or solution of common salt, gave the same constancy of operation.

813. Experiments of a similar kind were then made with bodies altogether in a different state, i. e. with *fused* chlorides, iodides, &c. I have already described an experiment with fused chloride of silver, in which the electrodes were of metallic silver, the one rendered negative becoming increased and lengthened by the addition of metal, whilst the other was dissolved and eaten away by its abstraction. This experiment was repeated, two weighed pieces of silver wire being used as the electrodes, and a volta-electrometer included in the circuit. Great care was taken to withdraw the negative electrode so regularly and steadily that the crystals of reduced silver should not form a *metallic* communication beneath the surface of the fused chloride. On concluding the experiment the positive electrode was re-weighed, and its loss ascertained. The mixture of chloride of silver, and metal, withdrawn in successive portions at the negative electrode, was digested in solution of ammonia, to remove the chloride, and the metallic silver remaining also weighed: it was the reduction at the *cathode*, and exactly equalled the solution at the *anode*; and each portion was as nearly as possible the equivalent to the water decomposed in the volta-electrometer.

814. The infusible condition of the silver at the temperature used, and the length and ramifying character of its crystals, render the above experiment difficult to perform, and uncertain in its results. I therefore wrought with a chloride of lead, using a green glass tube, formed as in fig. 17. A weighed platina wire was fused into the bottom of a small tube, as before described (789.). The tube was then bent to an angle, at about half an inch distance from the closed end; and the part between the angle and the extremity being softened, was forced upward, as in the figure, so as to form a bridge, or rather separation, producing two little depressions or basins *a*, *b*, within the tube. This arrangement was suspended by a platina wire, as before, so that the heat of a spirit-lamp could be applied to it, such inclination being given to it as would allow all air to escape during the fusion of the chloride of lead. A positive electrode was then provided, by binding up the end of a platina wire into a knob, and fusing about twenty grains of metallic lead on to it, in a small closed tube of glass, which was afterwards broken away. Being so furnished, the wire with its knob was weighed, and the weight recorded.

815. Chloride of lead was now introduced into the tube, and carefully fused. The leaded electrode was also introduced; after which the metal, at its extremity, soon melted. In this state of things the tube was filled up to *c* with melted chloride of lead; the end of the electrode to be rendered negative was in the basin *b*, and the electrode of melted lead was retained in the basin *a*, and, by connexion with the proper conducting wire of a voltaic battery, was rendered positive. A volta-electrometer was included in the circuit.

816. Immediately upon the completion of the communication with the voltaic battery, the current passed, and decomposition proceeded. No chlorine was evolved at

the positive electrode; but as the fused chloride was transparent, a button of alloy could be observed gradually forming and increasing in size at *b*, whilst the lead at *a* could also be seen gradually to diminish. After a time, the experiment was stopped; the tube allowed to cool, and broken open; the wires, with their buttons, cleaned and weighed; and their change in weight compared with the indication of the *volta-electrometer*.

817. In this experiment the positive electrode had lost just as much lead as the negative one had gained (795.), and the loss or gain was very nearly the equivalent of the water decomposed in the volta-electrometer, giving for lead the number 101·5. It is therefore evident, in this instance, that causing a *strong affinity*, or *no affinity*, for the substance evolved at the *anode*, to be active during the experiment (807.), produces no variation in the definite action of the electric current.

818. A similar experiment was then made with iodide of lead, and in this manner all confusion from the formation of a periodide avoided (803.). No iodine was evolved during the whole action, and finally the loss of lead at the *anode* was the same as the gain at the *cathode*, the equivalent number, by comparison with the result in the volta-electrometer, being 103·5.

819. Then protochloride of tin was subjected to the electric current in the same manner, using, of course, a tin positive electrode. No bichloride of tin was now formed (779. 790.). On examining the two electrodes, the positive had lost precisely as much as the negative had gained; and by comparison with the volta-electrometer, the number for tin came out 59.

820. It is quite necessary in these and similar experiments to examine the interior of the bulbs of alloy at the ends of the conducting wires; for occasionally, and especially with those which have been positive, they are cavernous, and contain portions of the chloride or iodide used, which must be removed before the final weight is ascertained. This is more usually the case with lead than tin.

821. All these facts combine into, I think, an irresistible mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377. 783.). They prove, too, that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and, further, that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances*, the whole combining together into *one series of definite electro-chemical actions* (505.). I do not mean to say that no exceptions will appear: perhaps some may arise, especially amongst substances existing only by weak affinity; but I do not expect that any will seriously disturb the result announced. If, in the well considered, well examined, and, I may surely say, well ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet, without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed if they should

present themselves at this, the opening of a new view of electro-chemical action; not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear.

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822. The doctrine of *definite electro-chemical action* just laid down, and, I believe, established, leads to some new views of the relations and classifications of bodies associated with or subject to this action. Some of these I shall proceed to consider.

823. In the first place, compound bodies may be separated into two great classes, namely, those which are decomposable by the electric current, and those which are not. Of the latter, some are conductors, others non-conductors, of voltaic electricity*. The former do not depend for their decomposability, upon the nature of their elements only; for, of the same two elements, bodies may be formed, of which one shall belong to one class and another to the other class; but probably on the proportions also (697.). It is further remarkable, that with very few, if any, exceptions (414. 691.), these decomposable bodies are exactly those governed by the remarkable law of conduction I have before described (394.); for that law does not extend to the many compound fusible substances that are excluded from this class. I propose to call bodies of this, the decomposable class, *Electrolytes* (664.).

824. Then, again, the substances into which these divide, under the influence of the electric current, form an exceedingly important general class. They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. I have proposed to call these bodies generally *ions*, or particularly *anions* and *cations*, according as they appear at the *anode* or *cathode* (665.); and the numbers representing the proportions in which they are evolved *electro-chemical equivalents*. Thus hydrogen, oxygen, chlorine, iodine, lead, tin, are *ions*; the three former are *anions*, the two metals are *cations*, and 1, 8, 36, 125, 104, 58, are their *electro-chemical equivalents* nearly.

825. A summary of certain points already ascertained respecting *electrolytes*, *ions*, and *electro-chemical equivalents*, may be given in the following general form of propositions, without, I hope, including any serious error.

826. i. A single *ion*, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary *ions*, and so subject to actual decomposition. Upon this fact is founded much of the proof adduced in favour of the new theory of electro-chemical decomposition, which I put forth in a former series of these Researches (518. &c.).

827. ii. If one *ion* be combined in right proportions (697.) with another strongly

* I mean here by voltaic electricity, merely electricity from a most abundant source, but having very small intensity.

opposed to it in its ordinary chemical relations, i. e. if an *anion* be combined with a *cation*, then both will travel, the one to the *anode*, the other to the *cathode*, of the decomposing body (530. 542. 547.).

828. iii. If, therefore, an *ion* pass towards one of the electrodes, another *ion* must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance (743.).

829. iv. A body decomposable directly by the electric current, i. e. an *electrolyte*, must consist of two *ions*, and must also render them up during the act of decomposition.

830. v. There is but one *electrolyte* composed of the same two elementary *ions*; at least such appears to be the fact (697.), dependent upon a law, that *only single electro-chemical equivalents of elementary ions can go to the electrodes, and not multiples.*

831. vi. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current when in combination (780.). It may act as an *ion*, going wholly to the *anode* or *cathode*, but does not yield up its elements, except occasionally by a secondary action. Perhaps it is superfluous for me to point out that this proposition has *no relation* to such cases as that of water, which, by the presence of other bodies, is rendered a better conductor of electricity, and *therefore* is more freely decomposed.

832. vii. The nature of the substance of which the electrode is formed, provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree (807. 813.); but it seriously influences, by secondary action (744.), the state in which the *ions* finally appear. Advantage may be taken of this principle in combining and collecting such *ions* as, if evolved in their free state, would be unmanageable *.

833. viii. A substance which, being used as the electrode, can combine altogether with the *ion* evolved against it, is also, I believe, an *ion*, and combines, in such cases, in the quantity represented by its *electro-chemical equivalent*. All the experiments I have made agree with this view; and it seems to me, at present, to result as a necessary consequence. Whether, in the secondary actions that take place, where the *ion* acts, not upon the matter of the electrode, but on that which is around it in the liquid (744.), the same consequence follows, will require more extended investigation to determine.

834. ix. Compound *ions* are not necessarily composed of electro-chemical equivalents of simple *ions*. For instance, sulphuric acid, boracic acid, phosphoric acid, are *ions*, but not *electrolytes*, i. e. not composed of electro-chemical equivalents of simple *ions*.

* It will often happen that the electrodes used may be of such a nature as, with the fluid in which they are immersed, to produce an electric current, either according with or opposing that of the voltaic arrangement used, and in this way, or by direct chemical action, may sadly disturb the results. Still, in the midst of all these confusing effects, the electric current, which actually passes in any direction through the decomposing body, will produce its own definite electrolytic action.

835. x. Electro-chemical equivalents are always consistent; i. e. the same number which represents the equivalent of a substance A when it is separating from a substance B, will also represent A when separating from a third substance C. Thus, 8 is the electro-chemical equivalent of oxygen, whether separating from hydrogen, or tin, or lead; and 103·5 is the electro-chemical equivalent of lead, whether separating from oxygen, or chlorine, or iodine.

836. xi. Electro-chemical equivalents coincide, and are the same, with ordinary chemical equivalents.

837. By means of experiment and the preceding propositions, a knowledge of *ions* and their electro-chemical equivalents may be obtained in various ways.

838. In the first place, they may be determined directly, as has been done with hydrogen, oxygen, lead, and tin, in the numerous experiments already quoted.

839. In the next place, from propositions ii. and iii., may be deduced the knowledge of many other *ions*, and also their equivalents. When chloride of lead was decomposed, platina being used for both electrodes (395.), there could remain no more doubt that chlorine was passing to the *anode*, although it combined with the platina there, than when the positive electrode, being of plumbago (794.), allowed its evolution in the free state; neither could there, in either case, remain any doubt, that for every 103·5 parts of lead evolved at the *cathode*, 36 parts of chlorine were evolved at the *anode*, for the remaining chloride of lead was unchanged. So also when in a metallic solution one volume of oxygen, or a secondary compound containing that proportion, appeared at the *anode*, no doubt could arise that hydrogen, equivalent to two volumes, had been determined to the *cathode*, although, by a secondary action, it had been employed in reducing oxides of lead, copper, or other metals, to the metallic state. In this manner, then, we learn from the experiments already described in these Researches, that chlorine, iodine, bromine, fluorine, calcium, potassium, strontium, magnesium, manganese, &c., are *ions*, and that their *electro-chemical equivalents* are the same as their *ordinary chemical equivalents*.

840. Propositions iv. and v. extend our means of gaining information. For if a body of known chemical composition is found to be decomposable, and the nature of the substance evolved as a primary or even a secondary result (743. 777.) at one of the electrodes, be ascertained, the electro-chemical equivalent of that body may be deduced from the known constant composition of the substance evolved. Thus, when fused protiodide of tin is decomposed by the voltaic current (804.), the conclusion may be drawn, that both the iodine and tin are *ions*, and that the proportions in which they combine in the fused compound express their electro-chemical equivalents. Again, with respect to the fused iodide of potassium (805.), it is an electrolyte; and the chemical equivalents will also be the electro-chemical equivalents.

841. If proposition viii. sustain extensive experimental investigation, then it will not only help to confirm the results obtained by the use of the other propositions, but will give abundant original information of its own.

842. In many instances, the *secondary results* obtained by the action of the evolving *ion* on the substances present in the surrounding liquid or solution, will give the electro-chemical equivalent. Thus, in the solution of acetate of lead, and, as far as I have gone, in other proto-salts subjected to the reducing action of the nascent hydrogen at the *cathode*, the metal precipitated has been in the same quantity as if it had been a primary product, (provided no free hydrogen escaped there,) and therefore gave as accurately the number representing its electro-chemical equivalent.

843. Upon this principle it is that secondary results may occasionally be used as measurers of the volta-electric current (706. 740.); but there are not many metallic solutions that answer this purpose well: for unless the metal is easily precipitated, hydrogen will be evolved at the *cathode* and vitiate the result. If a soluble peroxide is formed at the *anode*, or if the precipitated metal crystallize across the solution and touch the positive electrode, similar vitiated results are obtained. I expect to find in some vegetable salts, as the acetates of mercury and zinc, solutions favourable for this use.

844. After the first experimental investigations to establish the definite chemical action of electricity, I have not hesitated to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytical results. This, it is evident, may be done in a great number of cases, without using too much liberty towards the due severity of scientific research. The series of numbers representing electro-chemical equivalents must, like those expressing the ordinary equivalents of chemically acting bodies, remain subject to the continual correction of experiment and sound reasoning.

845. I give the following brief Table of *ions* and their electro-chemical equivalents, rather as a specimen of a first attempt than as anything that can supply the want which must very quickly be felt, of a full and complete tabular account of this class of bodies. Looking forward to such a table as of extreme utility (if well constructed) in developing the intimate relation of ordinary chemical affinity to electrical actions, and identifying the two, not to the imagination merely, but to the conviction of the senses and a sound judgement, I may be allowed to express a hope, that the endeavour will always be to make it a table of *real*, and not *hypothetical*, electro-chemical equivalents; for we shall else overrun the facts, and lose all sight and consciousness of the knowledge lying directly in our path.

846. The equivalent numbers do not profess to be exact, and are taken almost entirely from the chemical results of other philosophers in whom I could repose more confidence, as to these points, than in myself.

847. TABLE OF IONS.

Anions.

Oxygen	8	Cyanogen	26	Phosphoric acid	35·7	Citric acid	58
Chlorine	35·5	Sulphuric acid	40	Carbonic acid	22	Oxalic acid	36
Iodine	126	Selenic acid	64	Boracic acid	24	Sulphur (?)	16
Bromine	78·3	Nitric acid	54	Acetic acid	51	Selenium (?)	
Fluorine	18·7	Chloric acid	75·5	Tartaric acid	66	Sulpho-cyanogen	

Cations.

Hydrogen	1	Tin	57·9	Mercury	200	Strontia	51·8
Potassium	39·2	Lead	103·5	Silver	108	Lime	28·5
Sodium	23·3	Iron	28	Platina	98·6?	Magnesia	20·7
Lithium	10	Copper	31·6	Gold	(?)	Alumina	(?)
Barium	68·7	Cadmium	55·8			Protoxides generally.	
Strontium	43·8	Cerium	46	Ammonia	17	Quinia	171·6
Calcium	20·5	Cobalt	29·5	Potassa	47·2	Cinchona	160
Magnesium	12·7	Nickel	29·5	Soda	31·3	Morphia	290
Manganese	27·7	Antimony	64·6?	Lithia	18	Vegeto-alkalies generally.	
Zinc	32·5	Bismuth	71	Baryta	76·7		

848. This Table might be further arranged into groups of such substances as either act with, or replace, each other. Thus, for instance, acids and bases act in relation to each other; but they do not act in association with oxygen, hydrogen, or elementary substances. There is indeed little or no doubt that, when the electrical relations of the particles of matter come to be closely examined, this division must be made. The simple substances, with cyanogen, sulpho-cyanogen, and one or two other compound bodies, will probably form the first group; and the acids and bases, with such analogous compounds as may prove to be *ions*, the second group. Whether these will include all *ions*, or whether a third class of more complicated results will be required, must be decided by future experiments.

849. It is *probable* that all our present elementary bodies are *ions*, but that is not as yet certain. There are some, such as carbon, phosphorus, nitrogen, silicon, boron, alumium, the right of which to the title of *ion* it is desirable to decide as soon as possible. There are also many compound bodies, and amongst them alumina and silica, which it is desirable to class immediately by unexceptionable experiments. It is also *possible*, that all combinable bodies, compound as well as simple, may enter into the class of *ions*; but at present it does not seem to me probable. Still the experimental evidence I have is so small in proportion to what must gradually accumulate around, and bear upon, this point, that I am afraid to give a strong opinion upon it.

850. I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done, upon the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter; and it will probably lead us to the means by which we may enlighten that which is at present so obscure, and either fully demonstrate the truth of the idea, or develope that which ought to replace it.

851. A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent, or definite proportional, or atomic number of a body; for I have such conviction that the power which governs electro-decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to

feel no hesitation in believing that the latter must submit to them also. Such being the case, I can have no doubt that, assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c., notwithstanding that a very high authority doubles several of these numbers.

§ 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

852. The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of DALTON, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies: and when upon that knowledge comes the fact, that the electricity, which we appear to be capable of loosening from its habitation for a while, and conveying from place to place, *whilst it retains its chemical force*, can be measured out, and, being so measured, is found to be *as definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation*; we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.

853. Now it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. Let us, for instance, consider this and a few other points in relation to water. *One grain* of water acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire $\frac{1}{100}$ of an inch in thickness*, red hot, in the air during the whole time; and if interrupted anywhere by charcoal points, will produce a very

* I have not stated the length of wire used, because I find by experiment, as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in both cases. This I have proved by the volta-electrometer. I found that whether half an inch or eight inches were retained at one constant

brilliant and constant star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in the beautiful experiments of Mr. WHEATSTONE*, and to what I have said elsewhere on the relation of common and voltaic electricity (371. 375.), it will not be too much to say, that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolyzation, it has only separated the elements of a single grain of water.

854. On the other hand, the relation between the conduction of the electricity and the decomposition of the water is so close, that one cannot take place without the other. If the water is altered only in that small degree which consists in its having the solid instead of the fluid state, the conduction is stopped, and the decomposition is stopped with it. Whether the conduction be considered as depending upon the decomposition, or not (413. 703.), still the relation of the two functions is equally intimate and inseparable.

855. Considering this close and twofold relation, namely, that without decomposition transmission of electricity does not occur; and, that for a given definite quantity of electricity passed, an equally definite and constant quantity of water or other matter is decomposed; considering also that the agent, which is electricity, is simply employed in overcoming electrical powers in the body subjected to its action; it seems a probable, and almost a natural consequence, that the quantity which passes is the *equivalent* of, and therefore equal to, that of the particles separated; i. e. that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into the condition of *a current*, it would exactly equal the current required for the separation of that grain of water into its elements again.

856. This view of the subject gives an almost overwhelming idea of the extraordinary quantity or degree of electric power which naturally belongs to the particles of matter; but it is not inconsistent in the slightest degree with the facts which can be brought to bear on this point. To illustrate this I must say a few words on the voltaic pile†.

temperature of dull redness, equal quantities of water were decomposed in equal times in both cases. When the half-inch was used, only the centre portion of wire was ignited. A fine wire may even be used as a rough but ready regulator of a voltaic current; for if it be made part of the circuit, and the larger wires communicating with it be shifted nearer to or further apart, so as to keep the portion of wire in the circuit sensibly at the same temperature, the current passing through it will be nearly uniform.

* Literary Gazette, 1833, March 1 and 8. Philosophical Magazine, 1833, p. 204. L'Institute, 1833, p. 261.

† By the term voltaic pile, I mean such apparatus or arrangement of metals as up to this time have been called so, and which contain water, brine, acids, or other aqueous solutions or decomposable substances (476.), between their plates. Other kinds of electric apparatus may be hereafter invented, and I hope to construct some not belonging to the class of instruments discovered by VOLTA.

857. Intending hereafter to apply the results given in this and the preceding series of Researches to a close investigation of the source of electricity in the voltaic instrument, I have refrained from forming any decided opinion on the subject; and without at all meaning to dismiss metallic contact, or the contact of dissimilar substances, being conductors, but not metallic, as if they had nothing to do with the origin of the current, I still am fully of opinion with Davy, that it is at least continued by chemical action, and that the supply constituting the current is almost entirely from that source.

858. Those bodies which, being interposed between the metals of the voltaic pile, render it active, *are all of them electrolytes* (476.); and it cannot but press upon the attention of every one engaged in considering this subject, that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected, that one cannot happen without the other. This I have shown abundantly in water, and numerous other cases (402. 476.). If, then, a voltaic trough have its extremities connected by a decomposing body, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state may look at the part where the acid is acting upon the plates, and that where the current is acting upon the water, as the reciprocals of each other. In both parts we have the two conditions *inseparable in such bodies as these*, namely, the passing of a current, and decomposition; and this is as true of the cells in the battery as of the water cell; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination: decomposition is always included, and is, I believe, an essential chemical part.

859. But the difference in the two parts of the connected battery, that is, the decomposing or experimental cell, and the acting cells, is simply this. In the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition: in the latter we cause decompositions by ordinary chemical actions, (which are, however, themselves electrical,) and, as a consequence, have the electrical current; and as the decomposition dependent upon the current is definite in the former case, so is the current associated with the decomposition also definite in the latter (862. &c.).

860. Let us apply this in support of what I have surmised respecting the enormous electric power of each particle or atom of matter (856.). I showed in a former series of these Researches on the relation by measure of common and voltaic electricity, that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop of oil of vitriol and four ounces of distilled water at a temperature of about 60° Fahr., and connected at the other extremities by a copper wire eighteen feet long, and one eighteenth of an inch in thickness, yielded as much electricity in little more than three seconds of time as a Leyden battery charged by thirty turns of a very large and powerful plate electric machine

in full action (371.). This quantity, though sufficient if passed at once through the head of a rat or a cat to have killed it, as by a flash of lightning, was evolved by the mutual action of so small a portion of the zinc wire and water in contact with it, that the loss of weight sustained by either would be inappreciable by our most delicate instruments; and as to the water which could be decomposed by that current, it must have been insensible in quantity, for no trace of hydrogen appeared upon the surface of the platina during those three seconds.

861. What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water! We have already seen that it must be in quantity sufficient to sustain a platina wire $\frac{1}{114}$ of an inch in thickness, red hot, in contact with the air for three minutes and three quarters (853.), a quantity which is almost infinitely greater than that which could be evolved by the little standard voltaic arrangement to which I have just referred (860. 371.). I have endeavoured to make a comparison by the loss of weight of such a wire in a given time in such an acid, according to a principle and experiment to be almost immediately described (862.); but the proportion is so high, that I am almost afraid to mention it. It would appear that 800,000 such charges of the Leyden battery as I have referred to above, would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.

862. In further proof of this high electric condition of the particles of matter, and the *identity as to quantity, of that belonging to them with that necessary for their separation*, I will describe an experiment of great simplicity but extreme beauty, when viewed in relation to the evolution of an electric current and its decomposing powers.

863. A dilute sulphuric acid, made by adding about one part by measure of oil of vitriol to thirty parts of water, will act energetically upon a piece of plate zinc in its ordinary and simple state; but, as Mr. STURGEON has shown*, not at all, or scarcely so, if the surface of the metal has in the first instance been amalgamated; yet the amalgamated zinc will act powerfully with platina as an electromotor, hydrogen being evolved on the surface of the latter metal, as the zinc is oxidized and dissolved. The amalgamation is best effected by sprinkling a few drops of mercury upon the surface of the zinc, the latter being moistened with the dilute acid, and rubbing with the fingers so as to extend the liquid metal over the whole of the surface. Any mercury in excess forming liquid drops upon the zinc, should be wiped off†.

864. Two plates of zinc thus amalgamated were dried and accurately weighed; one, which we will call A, weighed 163·1 grains; the other, to be called B, weighed 148·3

* Recent Experimental Researches, &c., 1830, p. 74, &c.

† The experiment may be made with pure zinc, which, as chemists well know, is but slightly acted upon by dilute sulphuric acid in comparison with ordinary zinc, which during the action is subject to an infinity of voltaic actions. See DE LA RIVE on this subject, Bibliothèque Universelle, 1830, p. 391.

grains. They were about five inches long, and 0·4 of an inch wide. An earthenware pneumatic trough was filled with dilute sulphuric acid, of the strength just described (863.), and a gas jar, also filled with the acid, inverted in it*. A plate of platina of nearly the same length, but about three times as wide as the zinc plates, was put up into this jar. The zinc plate A was also introduced into the jar, and brought in contact with the platina, and at the same moment the plate B was put into the acid of the trough, but out of contact with other metallic matter.

865. Strong action immediately occurred in the jar upon the contact of the zinc and platina plates. Hydrogen gas rose from the platina, and was collected in the jar, but no hydrogen or other gas rose from *either* zinc plate. In about ten or twelve minutes, sufficient hydrogen having been collected, the experiment was stopped; during its progress a few small bubbles had appeared upon plate B, but none upon plate A. The plates were washed in distilled water, dried, and reweighed. Plate B weighed 148·3 grains, as before, having lost nothing by the direct chemical action of the acid. Plate A weighed 154·65 grains, 8·45 grains of it having been oxidized and dissolved during the experiment.

866. The hydrogen gas was next transferred to a water-trough and measured; it amounted to 12·5 cubic inches, the temperature being 52°, and the barometer 29·2 inches. This quantity, corrected for temperature, pressure, and moisture, becomes 12·15453 cubic inches of dry hydrogen at mean temperature and pressure; which, increased by one half for the oxygen that must have gone to the *anode*, i. e. to the zinc, gives 18·232 cubic inches as the quantity of oxygen and hydrogen evolved from the water decomposed by the electric current. According to the estimate of the weight of the mixed gas before adopted (791.), this volume is equal to 2·3535544 grains, which therefore is the weight of water decomposed; and this quantity is to 8·45, the quantity of zinc oxidized, as 9 is to 32·31. Now taking 9 as the equivalent number of water, the number 32·5 is given as the equivalent number of zinc; a coincidence sufficiently near to show, what indeed could not but happen, that for an equivalent of zinc oxidized an equivalent of water must be decomposed†.

867. But let us observe *how* the water is decomposed. It is electrolyzed, i. e. is decomposed voltaically, and not in the ordinary manner (as to appearance) of chemical decompositions; for the oxygen appears at the *anode* and the hydrogen at the *cathode* of the decomposing body, and these were in many parts of the experiment above an inch asunder. Again, the ordinary chemical affinity was not enough under the circumstances to effect the decomposition of the water, as was abundantly proved by the inaction on plate B; the voltaic current was essential. And to prevent any idea that the chemical affinity was almost sufficient to decompose the water, and that a smaller current of electricity might, under the circumstances, cause the hydrogen to

* The acid was left during a night with a small piece of unamalgamated zinc in it, for the purpose of evolving such air as might be inclined to separate, and bringing the whole into a constant state.

† The experiment was repeated several times with the same results.

pass to the *cathode*, I need only refer to the results which I have given (807. 813.) to show that the chemical action at the electrodes has not the slightest influence over the *quantities* of water or other substances decomposed between them, but that they are entirely dependent upon the quantity of electricity which passes.

868. What, then, follows as a necessary consequence of the whole experiment? Why, this: that the chemical action upon 32·31 parts, or one equivalent of zinc, in this simple voltaic circle, was able to evolve such quantity of electricity in the form of a current as, passing through water, should decompose 9 parts, or one equivalent of that substance: and, considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity which, being naturally associated with the particles of matter, gives them their combining power, is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that *the electricity which decomposes, and that which is evolved by the decomposition of, a certain quantity of matter, are alike.*

869. The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity, is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

870. I cannot refrain from recalling here the beautiful idea put forth, I believe, by BERZELIUS (703.) in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge which is at the moment taking place. The idea is in perfect accordance with the view I have taken of the *quantity* of electricity associated with the particles of matter.

871. In this exposition of the law of the definite action of electricity, and its corresponding definite proportion in the particles of bodies, I do not pretend to have brought, as yet, every case of chemical or electro-chemical action under its dominion. There are numerous considerations of a theoretical nature, especially respecting the compound particles of matter and the resulting electrical forces which they ought to possess, which I hope will gradually receive their development; and there are numerous experimental cases, as, for instance, those of compounds formed by weak affinities, the simultaneous decomposition of water and salts, &c., which still require

investigation. But whatever the results on these and numerous other points may be, I do not believe that the facts which I have advanced, or even the general laws deduced from them, will suffer any serious change; and they are of sufficient importance to justify their publication, even though much may remain imperfect or undone. Indeed, it is the great beauty of our science, CHEMISTRY, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the doors to further and more abundant knowledge, overflowing with beauty and utility to those who will be at the easy personal pains of undertaking its experimental investigation.

872. The definite production of electricity (868.) in association with its definite action proves, I think, that the current of electricity in the voltaic pile is sustained by chemical decomposition, or rather by chemical action, and not by contact only. But here, as elsewhere (857.), I beg to reserve my opinion as to the real action of contact, not having yet been able to make up my mind as to its being either an exciting cause of the current, or merely necessary to allow of the conduction of electricity, otherwise generated, from one metal to the other.

873. But admitting that chemical action is the source of electricity, what an infinitely small fraction of that which is active do we obtain and employ in our voltaic batteries! Zinc and platina wires, one eighteenth of an inch in diameter and about half an inch long, dipped into dilute sulphuric acid, so weak that it is not sensibly sour to the tongue, or scarcely to our most delicate test papers, will evolve more electricity in one twentieth of a minute (860.) than any man would willingly allow to pass through his body at once. The chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm (868. 861.). Nor is it merely true that the quantity is active; it can be directed and made to perform its full equivalent duty (867. &c.). Is there not, then, great reason to hope and believe that, by a closer *experimental* investigation of the principles which govern the development and action of this subtile agent, we shall be able to increase the power of our batteries, or invent new instruments which shall a thousandfold surpass in energy those which we at present possess?

874. Here for a while I must leave the consideration of the *definite chemical action of electricity*. But before I dismiss this series of experimental Researches, I would call to mind that, in a former series, I showed the current of electricity was also *definite in its magnetic action* (366. 367. 376. 377.); and, though this result was not pursued to any extent, I have no doubt that the success which has attended the development of the chemical effects is not more than would accompany an investigation of the magnetic phenomena.

Royal Institution,
December 31*st*, 1833.

VII. *On the Theory of the Moon.* By JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.*

Received November 30,—Read December 12, 1833.

M. POISSON having lately published a very important memoir on the Theory of the Moon, I am induced again to lay before the Society some remarks on this subject.

In this memoir M. POISSON expresses the three coordinates of the moon, namely, her true longitude, her distance, and her true latitude, in terms of the time. The reasons which he adduces for so doing are the same which led me also to deviate from the course which had always been pursued by mathematicians up to the time I commenced the investigation, and which consisted in employing the equations in which the true longitude is the independent variable.

Instead, however, of integrating the equations of motion by the method of indeterminate coefficients, as I have proposed, M. POISSON recommends the adoption of the method of the variation of the elliptic constants. Having reflected much upon this question before I entered upon the investigation, I will venture now to state the reasons which determined me not to employ the latter method.

It seems, in the first place, desirable to introduce uniformity in the methods employed in the theories of the perturbations of the moon, and of the planets, as far as this can be done without the sacrifice of any facility in the solution of the problem. It is not probable, however, that the tables of the planets will be deduced from the variations of their elements. In fact, as I have shown in a former paper, although the results obtained by either method are identical (as is also obvious *à priori*), it is only by numerous reductions that those deduced from the one method are convertible into those deduced from the other. Moreover, in order to obtain, through the variations of the elliptic elements, the inequalities of any given order in the coordinates, the development of the disturbing function must be carried one step further; so that, for example, in the theory of the moon, to obtain all the inequalities depending upon the fourth power of the moon's eccentricity, it would be necessary to obtain the terms depending upon the fifth power of the same eccentricity in the development of the disturbing function.

In the theory of the moon it is necessary to develop many terms in the disturbing function depending on the square of the disturbing mass, and even some depending on the cube; or, in other words, it is, as is well known, insufficient to substitute, in

the disturbing function, the elliptic values of the moon's coordinates. The variation of the disturbing function R may be obtained, according to my method, by substituting in the disturbing function the values of the moon's coordinates obtained by a second approximation, or, as in the method of M. Poisson, by substituting in the disturbing function the variations of the elliptic elements due to the disturbing force.

In the former case,

$$\delta R = r\frac{dR}{dr}\frac{\delta r}{r} + \frac{dR}{d\lambda}\delta\lambda + \frac{dR}{ds}\delta s.$$

In the latter,

$$\delta R = \frac{dR}{da}\delta a + \frac{dR}{de}\delta e + \frac{dR}{d\varpi}\delta\varpi + \frac{dR}{ds}\delta s + \frac{dR}{d\gamma}\delta\gamma + \frac{dR}{d\nu}\delta\nu + \frac{dR}{d\zeta}\delta\zeta,$$

ζ being equal to $\int n\,dt$.

In the former case it is necessary to multiply 3 series by 3 series, taken two and two; in the latter it is necessary to multiply 7 series by 7 series: and the labour required in the one is to that required in the other about in the same proportion of 3 to 7. The developments of $\delta\frac{dR}{de}$, $\delta\frac{dR}{ds}$, &c., which have to be separately obtained, require also the same labour.

It is important in a renewed investigation of the lunar theory, considered with a view of improving the lunar tables, to obtain by some independent method the expressions for the coordinates given finally in terms of the mean longitude by MM. Damoiseau and Plana; for when we consider the enormous number of terms which are necessary to be taken into account, and how difficult it is altogether to avoid error in numerical calculations, it is hardly to be expected that their results can be entirely free from error. If, however, the method of the variation of constants be adopted, after the variations of the elements have been obtained, it will require no small labour to effect the necessary substitutions in the elliptic expressions for the coordinates, so as finally to obtain the desired comparison. The quantity of labour necessary in order to bring to conclusion any solution of the problem is a very important consideration, as every additional work, whether in algebra or numbers, brings with it increased danger of mistakes, notwithstanding every care.

The preceding remarks, however, apply particularly to the determination of those inequalities which are not lowered by integration, that is, to almost all those which originate from the terms in R multiplied by $a,^{-s}$; but with respect to others, particularly those selected by M. Poisson, the method which he employs is very preferable.

Laplace, in the Mécanique Céleste, vol. iii. p. 171, alludes to an equation of long period of which the argument is twice the longitude of the moon's node, plus the longitude of her perigee, minus three times the longitude of the sun's perigee.

M. Poisson has shown that the coefficient of the corresponding argument in the development of the disturbing function equals zero. I shall now show that this im-

portant result may also be arrived at very simply by means of the method of developing R, I gave formerly*.

Employing the same notation,

$$\frac{\mathrm{d}R}{\mathrm{d}\gamma} = r' \frac{\mathrm{d}R}{\mathrm{d}\gamma}\frac{\mathrm{d}r'}{\mathrm{d}\gamma} + \frac{\mathrm{d}R}{\mathrm{d}\lambda'}\frac{\mathrm{d}\lambda'}{\mathrm{d}\gamma} + \frac{\mathrm{d}R}{\mathrm{d}s}\frac{\mathrm{d}s}{\mathrm{d}\gamma}$$

$$R = m_{,}\left\{ -\frac{1}{r_{,}} - \frac{r'^2}{4r_{,}^3}\{1 + 3\cos(2\lambda' - 2\lambda_{,}) - 2s^2\} \right.$$
$$\left. - \frac{r'^3}{8r_{,}^4}\{3(1 - 4s^2)\cos(\lambda' - \lambda_{,}) + 5\cos(3\lambda' - 3\lambda_{,})\}\right.$$

It is evident, by mere inspection of the value of R, that the term in question, of which the argument is $3\tau - \xi + 3\xi_{,} - 2\eta$, can only arise from the development of

$$-\frac{5r'^3}{8r_{,}^4}\cos(3\lambda' - 3\lambda_{,}),$$

so that we may consider $R = -\frac{5r'^3}{8r_{,}^4}\cos(3\lambda' - 3\lambda_{,})$ in the present investigation, and $\frac{\mathrm{d}R}{\mathrm{d}s} = 0$.

The argument† $3\tau - \xi + 3\xi_{,} - 2\eta$, can only be made up, therefore, of the arguments $3\tau - \xi + 3\xi_{,}$ and 2η, and $3\tau + 3\xi_{,}$ and $\xi + 2\eta$, by subtraction, if we limit ourselves to that part of the coefficient which is multiplied by $e\,e_{,}^3\gamma^2$.

It is therefore necessary to determine the coefficients of R corresponding to the arguments $3\tau + 3\xi_{,}$ and $3\tau - \xi + 3\xi_{,}$.

By the expression

$$\frac{\mathrm{d}R}{\mathrm{d}e_{,}} = r_{,}\frac{\mathrm{d}R}{\mathrm{d}r_{,}}\frac{\mathrm{d}r_{,}}{\mathrm{d}e_{,}} + \frac{\mathrm{d}R}{\mathrm{d}\lambda_{,}}\frac{\mathrm{d}\lambda_{,}}{\mathrm{d}e_{,}},$$

it is evident that the coefficient of $\cos 3\tau + 3\xi_{,}$ depends only upon the coefficients of $\cos 3\tau$, $\cos 3\tau + \xi_{,}$, and $\cos 3\tau + 2\xi_{,}$. On reference to the development of R‡, it will be found, that considering only these terms,

$$R = \frac{m_{,}a^3}{a_{,}^4}\left\{ -\underset{[116]}{\frac{5}{8}\cos 3\tau} + \underset{[120]}{\frac{5}{8}e_{,}\cos(3\tau + \xi_{,})} - \underset{[128]}{\frac{5}{64}e_{,}^2\cos(3\tau + 2\xi_{,})}\right\},$$

and since

$$r_{,}\frac{\mathrm{d}R}{\mathrm{d}r} = a_{,}\frac{\mathrm{d}R}{\mathrm{d}a_{,}}, \text{ and } \frac{\mathrm{d}R}{\mathrm{d}\lambda_{,}} = \frac{\mathrm{d}R}{\mathrm{d}\tau},$$

τ being used for $nt - n_{,}t$.

$$\frac{\mathrm{d}r_{,}}{r_{,}\mathrm{d}e_{,}} = -\cos\xi_{,} - \frac{3}{2}e_{,}\cos 2\xi_{,} - \frac{17}{8}e_{,}^2\cos 3\xi_{,},$$

$$\frac{\mathrm{d}\lambda_{,}}{\mathrm{d}e_{,}} = 2\sin\xi_{,} + \frac{5}{2}e_{,}\sin 2\xi_{,} + \frac{13}{4}e_{,}^2\sin 3\xi_{,},$$

neglecting terms which are not required.

* Philosophical Transactions, 1832, p. 606.

† I use the letters τ, ξ, $\xi_{,}$, and η, where formerly I used t, x, z, and y.

‡ Philosophical Transactions, 1831, p. 266.

$$3 \times \text{numerical coefficient of } \cos(3\tau + 3\xi_{,})$$

$$= -4 \times -\frac{5}{8} \times -\frac{17}{8} \times \frac{1}{2} - 4 \times \frac{5}{8} \times -\frac{3}{2} \times \frac{1}{2} - 4 \times -\frac{5}{64} \times -1 \times \frac{1}{2}$$

$$+ 3 \times -\frac{5}{8} \times \frac{13}{4} \times -\frac{1}{2} + 3 \times \frac{5}{8} \times \frac{5}{2} \times -\frac{1}{2} + 3 \times -\frac{5}{64} \times 2 \times -\frac{1}{2}$$

$$= \frac{-340 + 240 - 20 + 390 - 300 + 30}{128} = 0.$$

The first term of the coefficient, therefore, of $\cos(3\tau + 3\xi_{,})$ in the development of the disturbing function equals zero.

It is evident by the expression

$$\frac{dR}{de} = \frac{r\,dR}{dr}\frac{dr}{r\,de} + \frac{dR}{d\lambda}\frac{d\lambda}{de},$$

that the coefficient of $\cos(3\tau - \xi + 3\xi_{,})$ depends solely upon the coefficient of $\cos(3\tau + 3\xi_{,})$; and as this equals zero, the other must also equal zero; and as the coefficient of $\cos(3\tau - \xi + 3\xi_{,} - 2\eta)$ depends solely upon these two coefficients, it must also equal zero, which was the point to be ascertained.

M. Poisson has shown in the memoir before referred to, that the first term in the corresponding inequality of longitude depends only upon this coefficient in the development of R; the inequality is therefore insensible.

It follows equally that the coefficients of all arguments which result from any combination of $3\tau + 3\xi$, with any multiples of ξ and 2η are also equal to zero.

Note.—The expressions for $\frac{dr'}{r'\,d\gamma}$ and $\frac{d\lambda'}{d\gamma}$ I gave*, should be as follows:

$$\frac{dr'}{r'\,d\gamma} = -\frac{\gamma}{2} + \frac{\gamma}{2}(1 - 4e^2)\cos 2\eta - \gamma e \cos(\xi - 2\eta) + \gamma e \cos(\xi + 2\eta)$$
$$[62] \qquad [65] \qquad [66]$$
$$+ \frac{3}{8}\gamma e^2 \cos(2\xi - 2\eta) - \frac{13}{8}\gamma e^2 \cos(2\xi + 2\eta)$$
$$[77] \qquad [78]$$

$$\frac{d\lambda'}{d\gamma} = -\frac{\gamma}{2}(1 - 4e^2)\sin 2\eta + \gamma e \sin(\xi - 2\eta) - \gamma e \sin(\xi + 2\eta)$$
$$[62] \qquad [65] \qquad [66]$$
$$+ \frac{3}{8}e^2 \sin(2\xi - 2\eta) - \frac{13}{8}\gamma e^2 \sin(2\xi + 2\eta).$$
$$[77] \qquad [78]$$

The coefficient of the first term (argument 146) in the expression for $\frac{dR}{ds}$†, should be $\frac{408}{137}$ instead of $\frac{204}{137}$.

* Philosophical Transactions, 1832, p. 606.

† Ibid. p. 6.

VIII. *On the Theory of the Moon.* By JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.*

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WHEN I commenced the investigations relating to the theory of the moon which I have had the honour to communicate to the Society, I proposed to show how, by a different but more direct method, the numerical results given by M. DAMOISEAU might be obtained. The approximations were in fact carried much further by M. DAMOISEAU than had been done before, and the details which accompany M. DAMOISEAU's work evince at once the immense labour of the undertaking, and inspire confidence in the accuracy of the results offered. But the state of the question is now changed by the appearance of M. PLANA's admirable work, entitled "Théorie du Mouvement de la Lune," in which, although M. PLANA employs the same differential equations as those used by M. DAMOISEAU, and obtains in the same manner finally the expressions for the coordinates of the moon, in terms of the mean longitude by the reversion of series, yet M. PLANA's expressions have a very different analytical character and importance, from the circumstance that the author develops all the quantities introduced by integration, according to powers of the quantity called m, which expresses the ratio of the sun's mean motion to that of the moon. In this form of the expression the coefficients of the different powers of m, of the eccentricity, &c., are determinate, as are, for example, the numerical coefficients in the expression for the sine in terms of the arc, and other similar series. An inestimable advantage results from this procedure, which more than compensates for the great increase of labour it occasions, by diminishing the danger of neglecting any terms of the same order as those taken into account, and by affording the means of verifying many terms long before final and complete results shall have been obtained independently by myself or any other person. By treating the differential equations in which the time is the independent variable, as I have proposed, similar results to those of M. PLANA may be obtained directly; but the calculations which are required in either method are so prodigiously irksome and laborious, that until identical expressions have actually been obtained independently, to the extent of every sensible term, the theory of the moon cannot, I think, be considered complete. It might, indeed, be supposed that already, through the labours of mathematicians, from CLAIRAUT to the present time, the numerical values of the coefficients of the different inequalities were ascertained with sufficient accuracy for practical purposes, and that any further researches connected with the subject would be more likely to gratify curiosity than to lead to any useful result.

Astronomical observations are now made with so great precision, that the numerical values of the coefficients are wanted to at least the tenth of a second of space: very few, however, of the coefficients of MM. Damoiseau and Plana agree so nearly, and some differ much more, as may be seen in the following comparison of the numerical values of the coefficients of some of the arguments in the expression for the true longitude of the moon in terms of her mean longitude, being indeed those which differ the most.

	Argument.	Damoiseau.	Plana.
1	2τ	+ 2370·00	+ 2370·320
2	ξ	+22639·70	+22641·626
3	$2\tau - \xi$	+ 4589·61	+ 4585·648
4	$2\tau + \xi$	+ 192·22	+ 192·146
5	ξ_1	− 673·70	− 668·644
6	$2\tau - \xi_1$	+ 165·56	+ 165·850
7	$2\tau + \xi_1$	− 24·82	− 23·611
8	2ξ	+ 768·72	+ 769·477
9	$2\tau - 2\xi$	+ 211·57	+ 212·363
10	$2\tau + 2\xi$	+ 14·74	+ 14·119
11	$\xi + \xi_1$	− 109·27	− 111·099
12	$2\tau - \xi - \xi_1$	+ 207·09	+ 209·742
22	$2\tau + 3\xi$	+ 1·27	+ 3·309
24	$2\tau - 2\xi - \xi_1$	+ 8·99	+ 7·762
27	$2\tau - 2\xi + \xi_1$	+ 2·55	− 1·395
64	$2\tau + 2\eta$	− 5·75	− 3·376
65	$\xi - 2\eta$	+ 39·51	+ 37·191
110	$\tau - \xi + \xi_1$	+ 2·05	+ ·466
136	$4\tau - 2\xi$	+ 31·19	+ 34·518
	$4\tau - 2\xi - \xi_1$	+ 3·05	+ 1·197

When the coefficients of the inequalities have been determined analytically, it remains to determine with corresponding precision the numerical values of the arbitrary quantities m, e, and γ. The quantity m is already accurately known, but the quantities e and γ must be obtained from the coefficients of $\sin \xi$ in the expression for the longitude, and of $\sin \eta$ in the expression for the latitude, by the reversion of series; and it seems to me that the manner in which these arbitrary quantities are to be determined must be carefully and rigorously defined.

I propose to obtain the expression for the radius vector by means of the equation,

$$\frac{\mathrm{d}^2 . r^2}{2\,\mathrm{d}\,t^2} - \frac{\mu}{r} + \frac{\mu}{a} + 2\int \mathrm{d}\,R + r\frac{\mathrm{d}\,R}{\mathrm{d}\,r} = 0.$$

In order to integrate this equation, I suppose

$$\frac{a}{r} = 1 + r_0 + r_1 \cos 2\tau + e\left(1 - \frac{e^2}{8}\right)\cos\xi + \&c.$$

If r be used to denote the terms in r which are found in the elliptic expression, so that

$$\frac{a}{\mathrm{r}} = 1 + e\left(1 - \frac{e^2}{8}\right)\cos\xi + e^2\left(1 - \frac{e^2}{3}\right)\cos 2\xi + \frac{9}{8}e^3\cos 3\xi + \frac{4}{3}e^4\cos 4\xi + \&c.,$$

and

$$\frac{a}{r} = \frac{a}{\mathrm{r}} + a\delta\frac{1}{r}$$

$$\frac{d^2 . r^3}{2\,d\,t^2} - \frac{d^2 . r^2 \delta \frac{1}{r}}{d\,t^2} + \frac{3\,d^2 . r^4 \left(\delta \frac{1}{r}\right)^2}{2\,d\,t^2} - \frac{2\,d^2 . r^5 \left(\delta \frac{1}{r}\right)^3}{d\,t^2}$$

$$- \frac{\mu}{r} + \frac{\mu}{a} + 2\int d\,R + r\,\frac{d\,R}{d\,r} = 0.$$

Let $\mathfrak{r}_n$ be that part of the coefficient of the nth argument in the development of the quantity

$$- r^3 \delta \frac{1}{r} + \frac{3}{2} r^4 \left(\delta \frac{1}{r}\right)^2 + \&c.,$$

which corresponds to the argument of which n is the index, and let R_n be the coefficient corresponding to the argument of which n is the index in the development of R, R'_n the corresponding coefficient in the development of that part of $\delta\,d\,R$ which is multiplied by m, and only arises in the second approximation, with its sign changed, then the quantities r_n are given by equations similar to the following,

$$r_1 \left\{ \left\{ 1 + 3\,e^2 \left(1 + \frac{e^2}{8}\right) \right\} \{2 - 2\,m\}^2 - 1 \right\} = (2 - 2\,m)^2\,\mathfrak{r}_1$$

$$- 2 \left\{ \left\{ \frac{2}{2 - 2\,m} + 1 \right\} m^2\,R_1 + \frac{m^2}{(2 - 2\,m)}\,R'_1 \right\}$$

Passing over terms given by M. Plana and arising from the first approximation with which I agree, I come to r_{22}.

$$r_{22} \left\{ \{2 - 2\,m + 3\,c\}^2 - 1 \right\} = (2 - 2\,m + 3\,c)^2\,\mathfrak{r}_{22}$$

$$- 2 \left\{ \frac{2 + 3\,c}{(2 - 2\,m + 3\,c)} + 1 \right\} m^2\,R_{22}$$

$$\mathfrak{r}_{22} = \frac{3}{2} r_{10} - \frac{1}{16} r_1 \qquad r_{10} = \frac{7}{2} m^2 \qquad r_1 = m^2 \qquad *R_{22} = - \frac{25}{32}$$

$$r_{22} = \frac{25 . 3 . 7}{24 . 2 . 2} m^2 - \frac{25}{24 . 16} m^2 + \frac{2 . 2 . 25}{24 . 32} m^2 = \frac{2125}{384} m^2$$

M. Plana has $- \frac{1175}{384} m^2$

$$r_{25} \left\{ \{2 - m + 2\,c\}^2 - 1 \right\} = (2 - m + 2\,c)^2\,\mathfrak{r}_{25} - 2 \left\{ \frac{2 + 2\,c}{2 - m + 2\,c} + 1 \right\} m^2\,R_{25}$$

$$\mathfrak{r}_{25} = \frac{3}{2} r_{13} \qquad r_{13} = - \frac{33}{32} m^2 \qquad R_{25} = \frac{3}{8}$$

$$r_{25} = - \frac{16 . 3 . 33}{15 . 2 . 32} m^2 - \frac{2 . 2 . 3}{15 . 8} m^2 = - \frac{7}{4} m^2. \qquad \text{M. Plana has } \frac{7}{2} m^2$$

$$r_{45} \left\{ \{2 - m - 3\,c\}^2 - 1 \right\} = \{2 - m - 3\,c\}^2\,\mathfrak{r}_{45} - 2 \left\{ \frac{2 - 3\,c}{2 - m - 3\,c} + 1 \right\} m^2\,R_{45}$$

* Wherever I have found a disagreement with the result of M. Plana, as this might arise from an error in my development of R, I have verified the terms employed.

$$\mathfrak{r}_{45} = \frac{3}{2} r_{27} - \frac{1}{16} r_7 \qquad r_{27} = \frac{15}{8} m^2 \qquad r_7 = -\frac{m^2}{2} \qquad R_{45} = \frac{7}{64}$$

$$r_{45} = \frac{2.2.8}{3.15} m + \frac{m}{2.16.2} + \frac{2.2.7}{2.64} m = \frac{105}{64} m \qquad \text{M. Plana has } \frac{285}{64} m$$

$$r_{53} \left\{ \{c + 3m\}^2 - 1 \right\} = (c + 3m)^2 \mathfrak{r}_{53} - 2 \left\{ \frac{c}{c + 3m} + 1 \right\} m^2 R_{53}$$

$$\mathfrak{r}_{53} = \frac{3}{2} r_{35} \qquad r_{35} = -\frac{53}{16} m^2 \qquad R_{53} = \frac{53}{32}$$

$$r_{53} = -\frac{3.53}{6.2.16} m - \frac{2.2.53}{6.32} m = -\frac{127}{64} m \qquad \text{M. Plana has } -\frac{53}{32} m$$

$$r_{56} \{(c - 3m)^2 - 1\} = (c - 3m)^2 \mathfrak{r}_{56} - 2 \left\{ \frac{c}{c - 3m} + 1 \right\} m^2 R_{56}$$

$$\mathfrak{r}_{56} = \frac{3}{2} r_{35} \qquad R_{56} = \frac{53}{32}$$

$$r_{56} = \frac{3.53}{6.2.16} m + \frac{2.2.53}{6.32} = \frac{371}{192} m \qquad \text{M. Plana has } \frac{53}{64} m$$

The development of R which I gave*, results from the substitution of the elliptic values of the coordinates of the sun and moon in the disturbing function. The elliptic expression for the radius vector contains no term of which the argument is $\xi - 2\eta$, the longitude (λ') contains the term $+ \frac{3}{4} e \gamma^2 \sin(\xi - 2\eta)$. This is changed when the disturbing function is considered.

$$r_{65} \{(c - 2g)^2 (1 - 3r_0) - 1\} = (c - 2g)^2 \mathfrak{r}_{65} - 2 . 2 . m^2 R_{65}$$

$$\mathfrak{r}_{65} = \frac{3}{2} r_{62} \qquad r_{62} = \frac{m^2}{2} \qquad c = 1 - \frac{3}{4} m^2 \qquad g = 1 + \frac{3}{4} m^2$$

$$r_0 = \frac{m^2}{6} \qquad R_{65} = \frac{9}{8} + \frac{1}{2} r_{65} \qquad (c - 2g)\, 2\, (1 - 3r_0) = 1 + 4m^2$$

$$r_{65} = \frac{3}{4.2.2} - \frac{2.2}{4} \left(\frac{9}{8} + \frac{1}{2} r_{65} \right)$$

$$r_{65} = -\frac{5}{8}.$$

This term, produced by the disturbing force, although independent of m, together with the corresponding term in λ', renders in a certain sense incomplete the coefficients of all terms in my development of R, of which the arguments are any combinations of the quantity $\xi - 2\eta$.

$$r_{73} \{(2 - 3m - 2g)^2 - 1\} = (2 - 3m - 2g)^2 \mathfrak{r}_{73} - 2m^2 R_{73}$$

$$\mathfrak{r}_{73} = 0 \qquad R_{73} = -\frac{21}{16}$$

$$r_{73} = -\frac{2.21}{16} m^2 = -\frac{21}{8} m^2 \qquad \text{M. Plana has } \frac{7}{8} m - \frac{7}{8} m^2$$

* Philosophical Transactions, 1831, p. 263.

The term above, $-\frac{5}{8} e\gamma^2 \cos(\xi - 2\eta)$, introduces the term $+\frac{3}{4} e\gamma^2 \sin(\xi - 2\eta)$ in the longitude, instead of $-\frac{e\gamma^2}{2} \sin(\xi - 2\eta)$. The terms in R produced in consequence may easily be found from the formula

$$\delta R = -a\left(\frac{dR}{da}\right) r\delta\frac{1}{r} + \frac{dR}{d\tau}\delta\lambda$$

taking

$$r\delta\frac{1}{r} = -\frac{5}{8} e\gamma^2 \cos(\xi - 2\eta), \text{ and } \delta\lambda = \frac{5}{4} e\gamma^2 \sin(\xi - 2\eta)$$

and I find that R contains, instead of the terms corresponding to the same arguments given in the Philosophical Transactions, 1831, p. 263.

$$+\frac{15}{32} m^2 e\gamma^2 \cos(2\tau - \xi + 2\tau) - m^2 e\gamma^2 \cos(2\tau + \xi - 2\eta)$$
[68] [69]

$$+\frac{39}{32} m^2 e e_i \gamma^2 \cos(\xi + \xi_i - 2\eta) + \frac{105}{64} m^2 e e_i \gamma^2 \cos(2\tau - \xi - \xi_i + 2\eta)$$
[83] [86]

$$+\frac{33}{64} m^2 e e_i \gamma^2 \cos(e\tau + \xi + \xi_i - 2\eta) - \frac{15}{84} m^2 e e_i \gamma^2 \cos(2\tau - \xi + \xi_i + 2\eta)$$
[87] [92]

$$-\frac{231}{64} m^2 e e_i \gamma^2 \cos(2\tau + \xi - \xi_i - 2\eta)$$
[93]

The coefficient of arg. 77 is easily found as follows:

$$R = -\frac{r^2}{4r_i^3}\{1 + 3\cos(2\lambda - 2\lambda) - 2s^2\}.$$

This term can only arise from

$$-\frac{r^2}{4r_i^3}\{1 - 3s^2\}$$

$$= \frac{r^2}{2r_i^3}\delta\frac{1}{r} + \frac{3r^2}{4r_i^3}s^2.$$

In which expression it is sufficient to write for $\delta\frac{1}{r}$,

$$-\frac{5}{8} e\gamma^2 \cos(\xi - 2\eta)$$

and to make

$$*s^2 = -\frac{\gamma^2}{2}\cos 2\eta + \gamma^2 e\cos(\xi - 2\eta) + \frac{1}{4}\gamma^2 e^2 \cos(2\xi - 2\eta)$$
[65] [77]

which gives

$$R_{77} = \frac{3.5}{2.2.8} + \frac{3}{4.4} - \frac{3}{4} + \frac{3}{4.2.2.2} = 0.$$

* This is not the expression for s^2 in the elliptic movement: the last term is altered by the disturbing force.

When the elliptic values of the coordinates are substituted in the disturbing function, the term in question arises only from the expansion of the quantity

$$+\frac{3}{4}\frac{r^2}{r_i^3}s^2,$$

and in the elliptic motion

$$s^2 = -\frac{\gamma^2}{2}\cos 2\eta + \gamma^2 e\cos(\xi - 2\eta) - \frac{3}{8}\gamma^2 e^2\cos(2\xi - 2\eta) + \&c.$$

$$R_{77} = -\frac{3.3}{4.8} - \frac{3}{4} + \frac{3}{4.2.2.2} = -\frac{15}{16}.$$

Writing the index between brackets instead of the cosine of the corresponding argument, in order to save space.

$$-\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}t$$

$$= \frac{3}{4}\left\{1 - \frac{5}{2}e^2 - \frac{5}{2}e_,^2 - \frac{\gamma^2}{2}\right\}m^2\,[1] - \frac{9}{2}\left\{1 - \frac{13}{24}e^2 - \frac{5}{2}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e\,[3]$$

$$+\frac{1}{2}\left\{1 - \frac{19}{8}e^2 - \frac{5}{2}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e\,[3] \quad +\frac{21}{8}\left\{1 - \frac{5}{2}e^2 - \frac{123}{56}e_i^2 - \frac{\gamma^2}{2}\right\}m^2 e_,\,[6]$$

$$+\frac{3}{8}\left\{1 - \frac{5}{2}e^2 - \frac{e_i^2}{8} - \frac{\gamma^2}{2}\right\}m^2 e_,\,[7] \quad -\frac{15}{8}\left\{1 - \frac{5}{2}e_,^2 - \frac{\gamma^2}{2}\right\}m\,e^2\,[9]$$

$$+\frac{3}{8}\left\{1 - \frac{5}{2}e^2 - \frac{5}{2}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e^2\,[10] \quad -\frac{63}{4}\left\{1 - \frac{91}{128}e^2 - \frac{123}{56}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e e_,\,[12]$$

$$-\frac{1}{4}\left\{1 - \frac{19}{8}e^2 - \frac{e_i^2}{8} - \frac{\gamma^2}{2}\right\}m^2 e\,e_,\,[13] \quad +\frac{9}{4}\left\{1 - \frac{13}{24}e^2 - \frac{e_i^2}{8} - \frac{\gamma^2}{2}\right\}m^2 e\,e_,\,[15]$$

$$+\frac{7}{4}\left\{1 - \frac{19}{8}e^2 - \frac{123}{56}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e_,^2\,[16] \quad +\frac{51}{8}\left\{1 - \frac{5}{2}e^2 - \frac{115}{51}e_,^2 - \frac{\gamma^2}{2}\right\}m^2 e_,^2\,[18]$$

$$+\frac{5}{16}m\,e^3 - \frac{35}{8}m\,e_,^2 e_,\,[24] \quad -\frac{3}{16}m^2 e^2 e_,\,[25] \quad +\frac{15}{8}m\,e^2 e_,\,[27] \quad +\frac{21}{16}m^2 e^2 e_,\,[28]$$

$$-\frac{153}{4}m^2 e\,e_,^2\,[30] \quad +\frac{17}{4}m^2 e\,e_,^2\,[34] \quad +\frac{845}{64}m^2 e_,^3\,[36] \quad +\frac{m^2 e_i^3}{64}\,[37] \quad +\frac{3}{64}m^2 e^4\,[39]$$

$$+\frac{9}{32}m^2 e^4\,[40] \quad +\frac{49}{32}m^2 e^3 e_,\,[42] \quad +\frac{25}{32}m^2 e^3 e_,\,[43] \quad -\frac{7}{32}m^2 e^3 e_i\,[45]$$

$$+\frac{35}{32}m^2 e^3 e_i\,[46] \quad -\frac{255}{32}m^2 e^2 e_i^2\,[48] \quad +\frac{51}{16}m^2 e^2 e_,^2\,[52] \quad -\frac{2535}{64}m^2 e\,e_,^3\,[54]$$

$$-\frac{1}{96}m^2 e\,e_i^3\,[55] \quad -\frac{3}{32}m^2 e_,^3\,[57] \quad -\frac{15}{32}m^2 e_,^3\,[58] \quad +\frac{2453}{128}m^2 e_,^4\,[60] \quad -\frac{741}{128}m^2 e_i^4\,[70]$$

I have verified some of the terms in the expression for the reciprocal of the radius vector given by M. Plana, which depend on $\frac{a^3}{a_,^3}$, and arise from the second portion of R; very few, however, of these can be obtained without a further development of the disturbing function, in consequence particularly of the term

$$\frac{5}{4}\frac{a}{a_,}e_,\cos(\tau + \xi_,),$$

which is independent of m. In consequence of this term, all the terms in my development of R, of which the arguments are any combination of the quantity $\tau + \xi$, are incomplete.

When the terms depending on γ^2, and those depending on the square of the disturbing force, are neglected, the inequalities of longitude are given by the equation

$$\frac{\mathrm{d}\lambda}{\mathrm{d}t} = \frac{h}{r^2} - \frac{1}{r^2}\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\,\mathrm{d}t.$$

I find from this equation in λ_4 the term

$$\frac{175}{32} m e^2 = \left\{\frac{405}{32} m e^2 + \frac{15}{4} m e^2 + \frac{15}{8} m e^2\right\} \frac{1}{3},$$

instead of $\frac{195}{32} m^2$, according to M. Plana*.

λ_6 contains the term $-\frac{1353}{128} m^2 e_{,}^2$ instead of $-\frac{1253}{128} m^2 e_{,}^2$, for

R . . . . . $+\frac{369}{64} e_{,}^3 \cos(2\tau - \xi_{,})$

$\frac{a}{r}$ . . . . . $-\frac{123}{16} m^2 e_{,}^3 \cos(2\tau - \xi_{,})$

λ_6 . . . . . $\left\{-\frac{123}{8} m^2 - \frac{369}{64} m^2\right\} e_{,}^2 = -\frac{1353}{64} m^2 e_{,}^2.$

If the numerical coefficient of the corresponding term in the quantity $-\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\,\mathrm{d}t$, be called $\mathfrak{R}$, then I find

$$\lambda_{22} = \left\{2 r_{22} + r_{10} + r_{11} + \frac{9}{8} r_1 + \mathfrak{R}_{22} + \mathfrak{R}_{10} + \frac{5}{4}\mathfrak{R}_4 + \frac{13}{8}\mathfrak{R}_1\right\} \frac{1}{(2 - 2m + 3c)}$$

$$= \left\{\frac{2125}{192} m^2 + \frac{7}{2} m^2 + \frac{33}{16} m^2 + \frac{9}{8} m^2 + \frac{5}{16} m^2 + \frac{3}{8} m^2 + \frac{5}{8} m^2 + \frac{13.3}{8.4} m^2\right\} \frac{1}{5}$$

$$= \frac{779}{192} m^2.$$

M. Plana has $\frac{1093}{64} m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds 3″·309. M. Damoiseau has 1″·27; I obtain ·77″.

I find

$$\lambda_{25} = \left\{2 r_{25} + r_{13} + r_7 + \mathfrak{R}_{25} + \mathfrak{R}_{13} + \frac{5}{4}\mathfrak{R}_7\right\} \frac{1}{(2 - m + 2c)}$$

$$= \left\{-\frac{7}{2} m^2 - \frac{33}{32} m^2 - \frac{m^2}{2} - \frac{3}{16} m^2 - \frac{1}{4} m^2 + \frac{5.3}{4.8}\right\} \frac{1}{4} = -\frac{5}{4} m^2.$$

M. Plana has $-\frac{95}{64} m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds — ·087. M. Damoiseau has — ·19; I obtain — ·073.

* The figures are indistinct in the copy before me.

I find

$$\lambda_{28} = \left\{\frac{49}{2}m^2 + \frac{231}{32}m^2 + \frac{7}{2}m^2 + \frac{21}{16}m^2 + \frac{9}{4}m^2 + \frac{5.21}{4.8}m^2\right\}\frac{1}{4} = \frac{673}{64}m^2.$$

M. Plana has $\frac{665}{64}m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds $''\cdot607$. M. Damoiseau has $''\cdot90$; I obtain $''\cdot615$.

I find

$$\lambda_{49} = \left\{2r_{49} + r_{31} + r_{19} + \mathfrak{R}_{49} + \mathfrak{R}_{31} + \frac{5}{4}\mathfrak{R}_{19}\right\}\frac{1}{(2+2c)} = 0;$$

M. Plana has $-\frac{885}{64}m$.

I find

$$\lambda_{59} = 2r_{59} \times \frac{1}{4m} = \frac{591}{16}m, \qquad \text{M. Plana has } -\frac{77}{32}m;$$

I find

$$\lambda_{60} = \{2r_{60} + \mathfrak{R}_{60}\}\frac{1}{(2-6m)} = -\frac{55}{8}m.$$

These discordances will appear very trifling, considering the nature of the calculations; and it is by no means impossible, after all, that M. Plana may be right, and that the mistake may be with me, notwithstanding all the pains I have used.

Before the terms in the longitude can be arrived at which depend on γ^2, it is necessary to obtain the expression for the tangent of the latitude s: this may be done by means of the equations

$$\frac{d^2z}{dt^2} + \frac{\mu z}{r^3} + \frac{m_i z}{r_i^3} + \frac{3m_i z r' r\cos(\lambda' - \lambda_i)}{r_i^5} = 0$$

$$z = \frac{rs}{\sqrt{1+s^2}} \qquad \frac{z}{r} = s - \frac{s^3}{2} + \frac{3}{8}s^5 - \&c.$$

$$s = \frac{z}{r} + \frac{z^3}{2r^3} - \&c.$$

$$\tan^{-1}s = s - \frac{s^3}{3} + \frac{s^5}{5} - \&c.$$

It is, however, more convenient in the determination of s, to adhere to the method of Clairaut, that is, to the method adopted by M. Plana, notwithstanding the difficulties which occur in that method, and to which I have before alluded.

The following is the differential equation employed.

$$\left\{\frac{d^2s}{d\lambda'^2} + s\right\}\left\{1 - \frac{2}{h^2}\int r'^2\frac{dR}{d\lambda'}d\lambda'\right\}$$
$$+ \frac{r'^2}{h^2}\left\{(1+s^2)\left(\frac{dR}{ds}\right) - r's\left(\frac{dR}{dr'}\right) - \left(\frac{dR}{d\lambda'}\right)\frac{ds}{d\lambda'}\right\} = 0$$

Substituting in this equation in the terms multiplied by m_i, for s, $r\sin(g\lambda' - \nu)$, and for $\frac{ds}{d\lambda'}$, $-r\cos(g\lambda' - \nu)$, neglecting the square of the disturbing force and the cube of s, I obtain

$$\frac{d^2 s}{d\lambda^2} + s - \frac{3r^4}{2h^2 r_i^5}\{\sin(2\lambda - 2\lambda_i - g\lambda + \nu) - \sin(g\lambda - \nu)\}$$
$$- \frac{r^5}{8h^2 r_i^4}\left\{\frac{21}{2}\cos(\lambda - \lambda_i - g\lambda + \nu) + \frac{15}{2}\cos(\lambda - \lambda_i + g\lambda - \nu)\right.$$
$$\left. + 9\cos(3\lambda - \lambda_i - g\lambda + \nu) + 6\cos(3\lambda - 3\lambda_i + g\lambda - \nu)\right\}$$

The simplest method of substituting for λ_i in terms of λ seems to me to be by first obtaining expressions for $\cos\lambda_i$, $\sin\lambda_i$, $\cos 2\lambda_i$, $\sin 2\lambda_i$, in terms of $n_i t$. Having obtained these expressions, they may be reduced to terms of λ by LAGRANGE's theorem*; but when the higher powers of m are neglected, it is sufficient to write $m\lambda$ instead of $n_i t$.

$$\sin 2\lambda_i = (1 - 4e_i^2)\sin 2n_i t - 2e\sin(2n_i t - \xi_i) + 2e_i\sin(2n_i t + \xi_i)$$
$$+ \frac{3}{4}e_i^2\sin(2n_i t - 2\xi_i) + \frac{13}{4}e_i^2\sin(2n_i t + 2\xi_i) + \&c.$$

$$\cos 2\lambda_i = (1 - 4e_i^2)\cos 2n_i t - 2e_i\cos(2n_i t - \xi_i) + 2e_i\cos(2n_i t + \xi_i)$$
$$+ \frac{3}{4}e_i^2\cos(2n_i t - 2\xi_i) + \frac{13}{4}e_i^2\cos(2n_i t + 2\xi_i) + \&c.$$

Great facility results in the following substitutions in consequence of the coefficients being alike in the corresponding arguments of the expressions $\sin\lambda_i$, $\cos\lambda_i$; $\sin 2\lambda_i$, $\cos 2\lambda_i$, &c.; so

$$\sin(2\lambda - 2\lambda_i - g\lambda) = (1 - 4e^2)\{\sin(2\lambda - g\lambda)\cos 2m\lambda - \cos(2\lambda - g\lambda)\sin 2m\lambda\}$$
$$- 2e_i\{\sin(2\lambda - g\lambda)\cos(2m\lambda - c_i m\lambda) - \cos(2\lambda - g\lambda)\sin(2m\lambda - c_i m\lambda)\}$$
$$+ 2e_{,}\{\sin(2\lambda - g\lambda)\cos(2m\lambda + c_{,} m\lambda) - \cos(2\lambda - g\lambda)\sin(2m\lambda + c_{,} m\lambda)\}$$

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* By LAGRANGE's theorem, if

$$u = \theta - f\theta$$
$$\theta = u + fu + \frac{d.(fu)^2}{2\,du} + \frac{d^2(fu)^3}{2.3\,du^2} + \&c.$$

So
$$a_i^3 r_i^{-3} = 1 + \frac{3}{2}e_i^2\left(1 + \frac{5}{4}e_i^2\right) + 3e_i\left(1 + \frac{9}{8}e_i^2\right)\cos\xi_i$$
$$+ \frac{9}{2}e_i^2\left(1 + \frac{7}{9}e_i^2\right)\cos 2\xi_i + \frac{53}{8}e_i^3\cos 3\xi_i + \frac{77}{8}e_i^4\cos 4\xi_i$$

$$m\lambda = n_i t + 2me\left(1 - \frac{e^2}{8}\right)\sin\xi + \frac{5}{4}me^2\left(1 - \frac{11}{30}e^2\right)\sin 2\xi$$
$$+ \frac{13}{12}me^3\sin\xi + \frac{103}{96}me^4\sin 4\xi + \&c.$$

Hence evidently

$$a_i^3 r_i^{-3} = 1 + \frac{3}{2}e_i^2\left(1 + \frac{5}{4}e_i^2\right) + 3e_i\left(1 + \frac{9}{8}e_i^2\right)\cos(c_i m\lambda)$$
$$+ \frac{9}{2}e_i^2\left(1 + \frac{7}{9}e_i^2\right)\cos(2c_i m\lambda) + \frac{53}{8}e_i^3\cos(3c_i m\lambda) + \frac{77}{9}e_i^4\cos(4c_i m\lambda)$$

\+ terms multiplied by m, c_i may be considered as equal to unity.

$$+\frac{3}{4}e_,^2\{\sin(2\lambda-g\lambda)\cos(2m\lambda-2c_,m\lambda)-\cos(2\lambda-g\lambda)\sin(2m\lambda-2c_,m\lambda)\}$$

$$+\frac{13}{4}e_,^2\{\sin(2\lambda-g\lambda)\cos(2m\lambda+2c_,m\lambda)-\cos(2\lambda-g\lambda)\cos(2m\lambda+2c_,m\lambda)\}$$

$$=(1-4e_,^2)\sin(2\lambda-2m\lambda-g\lambda)$$
$$-2e_,\sin(2\lambda-2m\lambda+c_,m\lambda-g\lambda)+2e_,\sin(2\lambda-2m\lambda-c_,m\lambda-g\lambda)$$
$$+\frac{3}{4}e_,^2\sin(2\lambda-2m\lambda+2c_,m\lambda-g\lambda)+\frac{13}{4}e_,^2\sin(2\lambda-2m\lambda-2c_,m\lambda-g\lambda)$$

$$\frac{m_,r^4}{k^2r_,^3}=m^2\left\{1+\frac{2}{3}e_,^2+3e_,\cos c_,m\lambda+\frac{9}{2}e_,^2\cos 2c_,m\lambda\right\}$$

$$\{1+2e^2-4e\cos c\lambda+5e^2\cos 2c\lambda\}$$

$$\frac{3}{2}\frac{m_,r^4}{k^2r_,^3}=m^2\left\{\frac{3}{2}+\frac{9}{4}e_,^2+3e^2-6e_,\cos c\lambda+\frac{9}{2}e_,\cos c_,m\lambda+\frac{15}{2}e^2\cos 2c\lambda\right.$$
$$\left.-9e_,\cos(e\lambda-c_,m\lambda)-9ee_,\cos(e\lambda+c_,m\lambda)+\frac{27}{4}e_,^2\cos 2c_,m\lambda\right\}$$

All the terms which I have verified in the expression for the latitude in terms of the true longitude agree with those given by M. Plana.

If $\lambda=nt+\alpha$, then by Taylor's theorem,

$$s=(s)+\left(\frac{ds}{d\lambda}\right)\alpha+\left(\frac{d^2s}{d\lambda^2}\right)\frac{\alpha^2}{2}+\left(\frac{d^3s}{d\lambda^3}\right)\alpha^3+\&c.$$

(s) being the quantity arising from the substitution of nt for λ, in the expression for s in terms of λ. In this manner I found the same terms as those given by M. Plana, except $-\frac{15}{32}me^2\gamma\sin(2\tau-2\xi+\eta)$ instead of $-\frac{15}{64}me^2\gamma\sin(2\tau-2\xi+\eta)$, and $-\frac{3}{8}mee_,\gamma\sin(2\tau+\xi+\xi_,-\eta)$ instead of $\frac{1}{2}mee_,\gamma\sin(2\tau+\xi+\xi_,-\eta)$.

I next obtained s^2, in order to procure the terms in the longitude depending on γ^2. The quantity e in my notation does not accord with that quantity in the work of M. Plana, but with $e\left(1-\frac{\gamma^2}{4}\right)$; so that, in order to arrive at the same figures in some of the terms multiplied by γ^2, this circumstance must be attended to.

I find

$$\lambda_{70}=\left\{-\frac{39}{16}m^2-\frac{11}{16}m^2-\frac{33}{32}m^2-m^2-\frac{1}{4}m^2-\frac{3}{8}m^2\right\}\frac{1}{5}=-\frac{37}{32}m^2$$

instead of $-\frac{39}{32}m^2$, according to M. Plana; and I find

$$\lambda_{85}=-\left\{\frac{35}{8}m+\frac{7}{8}m-\frac{35}{16}m-\frac{7}{8}m-\frac{7}{16}m\right\}=-\frac{7}{4}m$$

instead of $\frac{7}{8}m$, according to M. Plana.

These are the only discrepancies which I have noted in the terms multiplied by γ^2 which I have examined. In making use of the development of R before alluded to,

$$1 - \frac{\gamma^2}{2} + \frac{7}{16}\gamma^4 \text{ is to be substituted for } \cos^4 \frac{\gamma}{2}.$$

The relation between the constants h and a is to be obtained from the equations

$$r^4 \frac{\mathrm{d}\lambda^2}{\mathrm{d}t^2} = h^2 - 2\int r^2 \frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}\lambda$$

$$\frac{r^2\mathrm{d}\lambda^2 + \mathrm{d}r^2}{\mathrm{d}t^2} - \frac{2\mu}{r} + \frac{\mu}{a} + 2\int \mathrm{d}R = 0;$$

where, for simplicity, I have neglected at present quantities depending on γ^2.

$$\mathrm{d}.\frac{1}{r} = -\frac{\mathrm{d}r}{r^2}$$

$$\mathrm{d}r^2 = r^4\left(\mathrm{d}.\frac{1}{r}\right)^2 = \left(\frac{1}{r}\right)^{-4}\left(\mathrm{d}.\frac{1}{r}\right)^2 = a^2(1-4r_0)\left\{\frac{e^2c^2n^2}{2} + \frac{e^2r_3^2n^2}{2}\right\}$$

$2\int \mathrm{d}R$ can give no constant term *; therefore, considering the constant part only of the equation above, since the coefficients corresponding to all the arguments must be separately identical,

$$h^2\left\{1 + 2r_0 + \frac{e^2}{2} + \frac{e^2r_3^2}{2}\right\} = a\mu\left\{1 + 2r_0 - (1-4r_0)\right\}\frac{e^2c^2}{2} + \frac{e^2r_3^2}{2}\Big\}$$

r_4^2, being already multiplied by m^4, is not to be taken into account in this approximation.

$$c^2\{1 - 3r_0\} - 1 + 2m^2 = 0 \qquad c^2 = 1 - \frac{7}{2}m^2$$

$$h^2\left\{1 - m^2 + \frac{e^2}{2} + \frac{33}{128}m^2e^2 - \frac{3}{2}m^2e_,^2\right\} = a\mu\left\{1 - \frac{e^2}{2} - m^2 - \frac{321}{128}m^2e^2 - \frac{3}{2}m^2e_,^2\right\}$$

$$h^2 = a\mu\left\{1 - \frac{e^2}{2} - m^2 - \frac{321}{128}m^2e^2 - \frac{3}{2}m^2e_,^2\right\}$$

$$\left\{1 + m^2 - \frac{e^2}{2} - \frac{33}{128}m^2e^2 + \frac{3}{2}m^2e_,^2 - m^2e^2\right\}$$

$$= a\mu\left\{1 - e^2 - \frac{241}{64}m^2e^2\right\}$$

$$h = a\mu\left\{1 - \frac{e^2}{2} - \frac{241}{128}m^2e^2\right\}$$

$$\frac{\mathrm{d}\lambda}{\mathrm{d}t} = \frac{h}{r^2} - \frac{1}{r^2}\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}t.$$

* This seems at variance with the equation of M. Plana, vol. i. p. 122,

$$-\frac{2a}{\sigma}\int \mathrm{d}^1\Omega = -\frac{m^2}{2}\left\{1 + \frac{1}{2}\gamma^2 + \frac{3}{2}e^2 + \frac{3}{2}e^2\gamma^2\cos(2\varpi - 2\theta) + \&c.\right\}$$

which equation I am unable to understand.

The second term on the right-hand side of this equation gives no constant quantity: hence

$$\frac{h}{r^3} = \sqrt{\frac{\mu}{a^3}}\left\{1 - \frac{e^2}{2} - \frac{241}{128}m^2e^2\right\}\left\{1 - m^2 + \frac{e^2}{2} + \frac{33}{128}m^2e^2 - \frac{3}{2}m^2e_i^2\right\}$$

$$\lambda = \sqrt{\frac{\mu}{a^3}}\left\{1 - m^2 - \frac{9}{8}m^2e^2 - \frac{3}{2}m^2e_i^2\right\}t + \&c.$$

If the constant quantity which multiplies t be called

$$n = \sqrt{\frac{\mu}{a^3}}$$

$$\sqrt{\frac{\mu}{a^3}}\left\{1 - m^2 - \frac{9}{8}m^2e^2 - \frac{3}{2}m^2e_i^2\right\} = \sqrt{\frac{\mu}{a^3}}$$

$$a = a\left\{1 + \frac{2}{3}m^2 + \frac{3}{4}m^2e^2 + m^2e_i^2\right\}$$

$$\frac{a}{r} = \frac{a\left\{1 + \frac{2}{3}m^2 + \frac{3}{4}m^2e^2 + m^2e_i^2\right\}^{-1}}{r} = 1 - \frac{m^2}{2} - \frac{3}{4}m^2e^2 - \frac{3}{4}m^2e_i^2.$$

Reverting to the equation

$$\frac{d\lambda}{dt} = \frac{h}{r^2} - \frac{1}{r^2}\int\frac{dR}{d\lambda}dt.$$

The second term of this equation gives no term multiplied by $\cos\xi$; therefore,

$$d\lambda = n\,dt + \frac{2h}{a^2}(1 + r_0)\,e\cos\xi\,dt$$

$$= n\,dt + 2n\left(1 - \frac{m^2}{2}\right)(1 + m^2)\,e\cos\xi\,dt$$

$$c = 1 - \frac{7}{4}m^2 \qquad cn = cn \qquad c = 1 - \frac{3}{4}m^2$$

$$\lambda = nt + 2\left(1 + \frac{3}{4}m^2 + \frac{m^2}{2}\right)e\sin\xi$$

$$\lambda = nt + 2\left(1 + \frac{5}{4}m^2\right)e\sin\xi + \&c.$$

$$\frac{a}{r} = 1 + \frac{m^2}{6} + \frac{m^2e_i^2}{4} + e\left(1 + \frac{2}{3}m^2\right)\cos\xi + \&c.$$

These are the expressions for λ and for $\frac{1}{r}$, when the quantity e is retained; but if the coefficient of $\sin\xi$, in the expression for λ, be called $2e$, after the manner adopted for the planets in the first volume of the Mécanique Céleste, so that

$\lambda = nt + 2e\sin\xi + \&c.$, then

$$\frac{a}{r} = 1 + \frac{m^2}{6} + \frac{m^2e_i^2}{4} + e\left(1 - \frac{7}{12}m^2\right)\cos\xi + \&c.$$

As the preceding results do not quite agree with those of M. Plana, I shall endeavour to show how they may be obtained from the same equations which he employs.

$$\frac{d^2 \cdot \frac{1}{r}}{d\lambda^2} + \frac{1}{r} - \frac{1}{h^2}\left\{1 + 2\int r^2 \frac{dR}{d\lambda}\, d\lambda\right\} - \frac{r}{h^2}\left\{r\frac{dR}{dr} - \frac{1}{r}\frac{dR}{d\lambda}\cdot\frac{dr}{d\lambda}\right\} = 0$$

$$R = -\frac{r^3}{4r_1^3}\left\{1 + 3\cos(2\lambda - 2\lambda_1)\right\}$$

In order to integrate the preceding differential equation, let

$$\frac{h^2}{r} = \mu\{1 + r_0 + r_1\cos(2\lambda - 2m\lambda) + e\cos(c\lambda - \varpi) + er_3\cos(2\lambda - 2m\lambda - c\lambda + \varpi)\}$$
$$[0] \qquad [1] \qquad [2] \qquad [3]$$

The letters r_0, r_1, &c., being now used in a somewhat different sense to heretofore, having now reference to the expression for $\frac{1}{r}$ in terms of the true longitude.

$$\frac{1}{r}^{-3} = \frac{h^6}{\mu^3}(1 + 3e^2 + \&c.)$$

Neglecting e_1^2,

$$r^0 + \frac{h^6}{2a_1^3}\frac{m_1}{\mu}(1 + 3e^2) = 0.$$

Substituting in this equation for h its elliptic value which is allowable, $r_0 = -\frac{m^2}{2}$, also $r_3 = \frac{15}{8}m$,

$$\left(\frac{1}{r}\right)^{-2} = \frac{h^4}{\mu^2}\left\{1 + m^2 + \frac{225}{128} \times 3m^2e^2 + \frac{3e^2}{2} + 3m^2e^2\right\}$$

$$= \frac{h^4}{\mu^2}\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\}$$

$$\frac{dt}{d\lambda} = \frac{r^2}{h} = \frac{h^3}{\mu^3}\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\} = \sqrt{\frac{a^3}{\mu}}$$

$$h^2 = a\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\}^{-\frac{2}{3}}$$

$$= a\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2e^2\right\}$$

$$= a\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2e^2\right\}\left\{1 + \frac{2m^2}{3} + \frac{3}{4}m^2e^2\right\}$$

$$= a\left\{1 - e^2 - \frac{241}{64}m^2e^2\right\} \text{ as before.}$$

$$\frac{h^2}{r} = \frac{a\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2 e^2\right\}}{r}$$

$$= 1 - \frac{m^2}{2} + e\cos(c\lambda - \varpi) + \&c.$$

$$\frac{a}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{899}{192}m^2 e^2 + e\left(1 + \frac{2}{3}m^2\right)\cos(c\lambda - \varpi) + \&c.$$

My letter a appears to correspond with a $(1 + p)$, or a in M. PLANA's notation.

n	n
c	c
μ	σ
R	Ω
$\frac{1}{r^3}$	u
λ'	v.

Putting $e\left(1 - \frac{m^2}{2}\right)$ instead of e in the various expressions found above,

$$\lambda = n\,t + 2\left(1 + \frac{3}{4}m^2\right)e\sin\xi,$$

which then so far agrees with the expression of M. PLANA*, and I then find

$$\frac{a}{r} = 1 + \frac{m^2}{6} + \frac{m^2 e_i^2}{4} + e\left(1 + \frac{m^2}{6}\right)\cos\xi + \&c.$$

$$\frac{a}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{707}{192}m^2 e^2 + e\left(1 + \frac{m^2}{6}\right)\cos(c\lambda - \varpi).$$

M. PLANA has

$$\frac{a}{r} = 1 + \frac{m^2}{6} - \frac{45}{16}m^2 e^2 + \frac{m^2 e_i^2}{4} + e\left(1 + \frac{m^2}{6}\right)\cos\xi + \&c.\dagger$$

$$\frac{a}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{167}{192}m^2 e^2 + e\left(1 + \frac{m^2}{6}\right)\cos(c\lambda - \varpi) + \&c.\ddagger$$

which equations do not agree with those I have found. I am, however, well aware how difficult it is to escape error in these inquiries, and wish to be understood as not offering any of the results contained in this paper too confidently.

Wherever I presumed to have arrived at figures differing from those of M. PLANA, I verified afresh all the steps of the process contained in previous papers, particularly the corresponding term in the development of R. Thus I have found by means of the expressions given §, that three times the numerical coefficient of $e_i^3\cos(2\tau - \xi_i)$

* Vol. i. p. 574. † Vol. i. p. 664. ‡ Vol. i. p. 636.

§ Philosophical Transactions, 1832, p. 601.

in the development of R,

$$= \frac{63}{16} + \frac{81}{64} + \frac{45}{16} + \frac{27}{32} - \frac{153}{16} + \frac{18}{32} + \frac{30}{8} + \frac{30}{32} + \frac{204}{16} = \frac{1107}{64}.$$

The coefficient in question $= \frac{369}{64} = \frac{21}{8} \times \frac{153}{56}$.

But I have found that the following corrections are required. For

$$+ \frac{3}{8}\left\{1 - \frac{5}{2}e^2 - 4e_i^2\right\} \cos^4 \frac{i}{2} \frac{a^2}{a_i^3} e_i \cos(2\tau + \xi_i) \qquad [7]$$

read

$$+ \frac{3}{8}\left\{1 - \frac{5}{2}e^2 - \frac{e_i^2}{8}\right\} \cos^4 \frac{i}{2} \frac{a^2}{a_i^3} e_i \cos(2\tau + \xi_i)$$

and for

$$-\frac{51}{64}\frac{a^2}{a_i^3} e_i^2 \gamma^2 \cos(2\xi_i - 2\eta) \quad [95] \qquad -\frac{45}{64}\frac{a^2}{a_i^3} e_i^2 \gamma^2 \cos(2\xi_i + 2\eta) \quad [96] \qquad -\frac{195}{64}\frac{a^2}{a_i^3} e_i^2 \gamma^2 \cos(2\tau - 2\xi_i - 2\eta) \quad [97]$$

read

$$-\frac{27}{16}\frac{a^2}{a_i^3} e_i^2 \gamma^2 \cos(2\xi_i - 2\eta) - \frac{27}{16} e_i^2 \gamma^2 \cos(2\xi_i + 2\eta) - \frac{51}{16}\frac{a^2}{a_i^3} e_i^2 \gamma^2 \cos(2\tau - 2\xi_i - 2\eta).$$

IX. *On the Tides.* *By* J. W. Lubbock, *Esq. V. P. and Treas. R.S.*

Received December 16, 1833,—Read February 20, 1834.

I HAVE already presented to the Society some tables exhibiting results obtained by the discussion of many observations of the tides made at the London Docks. I have now to communicate other tables, also calculated, according to my instructions, by Mr. Dessiou. In those already published in the Philosophical Transactions, which have reference to the corrections due to the influence of the parallax and declination of the moon, Mr. Dessiou employed only observations of the tides made between conjunction and opposition; but considering it of importance to establish these corrections upon a greater number of observations, and also to ascertain whether any appreciable difference existed, Mr. Dessiou undertook to obtain similar corrections from observations made between opposition and conjunction. The difference, if there be any, is very small.

It formerly escaped my notice, that the correction due to the influence of the moon's declination is mixed up with that for the calendar months: but the inclination of the moon's orbit to the ecliptic is small; and when observations are considered throughout an entire revolution of her node, she may be taken to move in the ecliptic; so that her time of transit being given on any day of the year, her declination is also given. This suggestion was made to me by Mr. Whewell, and I have endeavoured to remedy this difficulty, and have now calculated the correction for the calendar months roughly, which may serve until there has been time to obtain it with greater precision.

My corrections for the influence of the moon's parallax and declination may, I think, be safely adopted; and I doubt if they be susceptible of much further improvement. Their discrepancy from the theory of Bernoulli is worthy of remark, as may be seen in the following Table, which gives Bernoulli's correction for the influence of the moon's parallax, and that which Mr. Dessiou has calculated.

Time of Moon's Transit.	Bernoulli	Observation.	Bernoulli	Observation.	Time of Moon's Transit.
	Moon's Horizontal Parallax.				
	54′		60′		
0	+ 4	+12	− 4	−11	0
1	+ 2	+11	− 2	− 9	1
2	0	+ 7	0	− 7	2
3	− 2	+ 7	+ 2	− 6	3
4	− 4	+ 4	+ 4	− 6	4
5	− 7	+ 1	+ 5	− 3	5
6	− 9	+ 1	+ 7	− 3	6
7	− 9	+ 6	+ 6	− 7	7
8	0	+14	0	−11	8
9	+ 9	+18	− 6	−15	9
10	+ 9	+15	− 7	−16	10
11	+ 7	+13	− 5	−14	11

The correction for parallax 57′ being considered as given by the semimenstrual inequality, Bernoulli's correction for the influence of the moon's declination is equally erroneous; but his accurate determination of the law of the semimenstrual inequality is one of the most important results ever obtained *à priori* by means of the theory of universal gravitation.

The theory of the tides is now, as Mr. Whewell remarks, in the state which that of the motions of the moon and planets presented about a century ago; and unless considerable exertions be made, it may so continue for many years to come. The tables of the planets have only acquired their present accuracy through the liberal encouragement of learned bodies, and of some of the governments of Europe; nor can tables of the tides, adapted to the present state of science, be now constructed, unless a very considerable expense be incurred, from the immense labour required.

In discussing tide observations, when the greatest possible accuracy is desired, and when the correctness of the observations appears to warrant such a nicety, in order to obtain any particular correction, all known approximate corrections of a different nature should first be separately applied to each observation with a contrary sign.

So, in the determination of the correction for the calendar months, it would be well to correct all the observations first for parallax and declination; and this consideration should be particularly attended to, where it is practicable, in attempting to determine, from a few observations, the establishment of any port and the semimenstrual inequality. The best method of verifying my tables would be to determine by their means the times and heights of high water at the London Docks for nineteen years, and then to classify the transits of the moon with the errors of those determinations, according to calendar months, and according to parallaxes and declinations: the average error corresponding to each calendar month would be the error of the Table* for the calendar months, and so for the rest.

* I allude now more particularly to that which accompanies this paper, not to any given previously.

Mr. W. Peirce, of the London Docks, under whose care the observations have been made, has kindly communicated to me the following information.

The observations were originally instituted at the instigation of Mr. W. Vaughan, of Fenchurch-street, then one of the directors, in consequence of having seen an account of the tides kept at Liverpool. The time was always taken by Wapping church clock, which is considered in Wapping as good a timekeeper as any in London. The observations, previously to the opening of the Docks, viz. from 1801 to January 1805, were taken by Mr. Peirce in the day, and by a foreman in the night. After the Docks opened, from 1805 to 1828, inclusive, they were taken by the watchman in the day, and by Mr. Peirce and two foremen, who attended alternately, fourteen nights each. From 1823 up to the present time they are taken in the day by the foreman at the entrance lock, in the night by two foremen alternately. The time has, at my instigation, been more particularly attended to since I had the accounts (1829), when Mr. Peirce gave the foreman charge to be particularly careful. The heights, previously to the opening of the Docks (viz. 1801 to 1805), were taken by the averaged eighteen-feet tide at the Trinity marks, or, as it is called, Trinity datum. The marks were fixed in a wall where the entrance now is. The lock being made five feet deeper than Trinity datum, there are twenty-three feet at the lock when the water is at the eighteen-feet mark Trinity datum; therefore the difference between the lines from which the heights are reckoned in the books containing the observations is five feet; so that eighteen feet previously to 1805 is the same height or depth as twenty-three feet after that time. In similar accounts of tide observations, the initials or the name at full length of the observer should be affixed to each observation, in order to afford a check upon the care with which they are made.

Sir John Hall has kindly favoured me with the following information relative to the influence of the wind upon the tides in the port of London. Sir John Hall procured the joint opinion of some nautical men, including the dock-master of the St. Katharine Docks, and the senior harbour-master of the port. The following is the result of their sentiments respecting the influence of the wind upon the tides in the river Thames.

During strong north-westerly gales, the tide marks high water earlier than otherwise, and does not give so much water, whilst the ebb-tide runs out later, and marks lower; but upon the gales abating, and the weather moderating, the tides put in, and rise much higher, whilst they also run longer before high water is marked, and with more velocity of current, nor do they run out so long or so low. The reason assigned for all this is, that the strong north-west winds drive the sea along the Dutch coast, through the straits of Dover, and consequently away from the mouth of the Thames; so that the tides, during north-west winds, are always much higher (producing frequently ruinous flooding,) on the Dutch than upon the English coast. A south-westerly gale has a contrary effect generally, and an easterly one gives some water; but the tides, in all these cases, always improve the moment the weather moderates.

This is the opinion of those most competent to form one, from their daily experience, and is no doubt correct. The subject is one of considerable importance, as regards the accuracy of which tide predictions are susceptible, and merits further inquiry, in order to ascertain, if possible, the error which may be expected for a wind of given force and direction.

In order to obtain the correction for the calendar months, I begin by forming the following Table, which gives the moon's declination roughly, but sufficiently near for my purpose, in different months of the year, supposing her to move in the ecliptic.

TABLE A.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	°	°	°	°	°	°	°	°	°	°	°	°	h
0	21	13	2	10	19	23	22	14	3	8	18	23	0
1	18	6	4	15	22	23	19	8	4	14	21	23	1
2	11	0	10	19	23	22	14	2	10	18	23	22	2
3	8	6	16	22	23	19	9	4	15	21	23	19	3
4	3	12	20	23	22	14	2	10	19	23	22	15	4
5	5	17	22	23	19	9	4	15	22	23	19	9	5
6	11	20	23	22	15	3	10	19	23	22	15	3	6
7	16	23	23	19	10	4	15	22	23	19	10	3	7
8	20	23	21	14	3	10	19	23	23	16	4	9	8
9	22	23	18	9	3	15	22	23	19	10	2	15	9
10	23	20	14	3	9	19	23	21	15	4	9	19	10
11	21	17	9	4	15	22	23	18	9	2	14	22	11

The difference between the interval from the mean time of the moon's transit and the time of high water, and the mean interval, is according to the Table given in the Philosophical Transactions for 1831, p. 412.

TABLE B.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 5	+ 2	+ 4	+ 7	− 2	− 7	− 4	0	+ 9	+ 7	− 3	− 7	0
1	− 3	+ 3	+ 6	+ 4	− 3	− 7	− 1	+ 8	+ 8	+ 3	− 8	− 7	1
2	− 1	+ 3	+ 8	+ 3	− 6	− 6	− 1	+11	+ 8	+ 2	−10	− 7	2
3	+ 2	+ 6	+ 5	+ 1	− 9	− 5	+ 2	+10	+ 7	− 4	−11	− 8	3
4	+10	+ 7	+ 2	− 6	− 8	− 1	+10	+12	+ 4	− 9	−14	− 3	4
5	+13	+ 3	− 4	−12	− 9	+ 5	+15	+ 9	− 3	−15	−11	+ 1	5
6	+16	− 2	−12	−14	− 4	+14	+18	+ 5	−10	−18	− 4	+13	6
7	+13	−11	−22	−12	+ 7	+21	+14	− 5	−19	−14	+ 8	+16	7
8	0	−27	−26	− 4	+14	+16	+ 5	−11	−13	+ 6	+18	+14	8
9	− 7	−23	− 9	+ 5	+13	+ 3	− 8	−11	− 1	+13	+15	+ 6	9
10	− 8	− 8	+ 2	+ 6	+ 7	− 3	−11	− 5	+ 4	+14	+ 5	− 4	10
11	− 6	− 1	+ 3	+ 6	+ 4	− 7	− 7	− 2	+ 7	+ 8	+ 3	− 3	11

The number −3, January, moon's transit 1^h, was inferred by subtracting $1^h\,39^m$ from $1^h\,42^m$, column A.* Column A. is the semimenstrual inequality + a constant. If we suppose the proper argument of this inequality to be the apparent time of the

* Philosophical Transactions, 1831, p. 401.

moon's transit, as theory suggests, the equation of time for the middle of January being + 10m, then 1h 39m should be subtracted from 1h 44m·8, the semimenstrual inequality corresponding to the moon's transit at 50m. This gives — 2·8 to be added to — 3. I therefore formed the following Table, which gives the quantity to be added for a given equation of time, in order to reduce the Table B. to what it should be, having the argument of the moon's transit in apparent time.

TABLE C.

Transit. Moon's	Equation of Time.		
	+ 5	+ 10	+ 15
h	m	m	m
0	−1·1	−2·3	−3·3
1	−1·2	−2·4	−3·6
2	−1·3	−2·7	−4·0
3	−1·3	−2·6	−3·9
4	− ·9	−1·8	−2·7
5	− ·5	−1·0	−1·5
6	0	0	0
7	+1·9	+3·9	+5.8
8	+2·8	+5·7	+8·4
9	+2·1	+4·2	+6·3
10	0	0	0
11	− ·7	−1·4	−2·1

By interpolation I then formed the following Table, taking the equation of time for the middle of the month.

TABLE D.

Time of Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Time of Moon's Transit.
	+10	+14	+ 9	0	− 4	0	+ 5	+ 4	− 5	−14	−15	− 4	
h													h
0	−2·3	−3·2	−1·9		+ ·9		−1·1	− ·9	+1·1	+3·2	+3·3	+ ·9	0
1	−2·4	−3·3	−2·1		+ ·9		−1·2	− ·9	+1·2	+3·3	+3·6	+ ·9	1
2	−2·7	−3·7	−2·4		+1·0		−1·3	−1·0	+1·3	+3·7	+4·0	+1·0	2
3	−2·6	−3·6	−2·3		+ ·9		−1·3	−1·0	+1·3	+3·6	+3·9	+ ·9	3
4	−1·8	−2·5	−1·6		+ ·7		− ·9	− ·7	+ ·9	+2·5	+2·7	+ ·7	4
5	−1·0	−1·4	− ·9		+ ·4		− ·5	− ·4	+ ·5	+1·4	+1·5	+ ·4	5
6	0	0	0		0		0	0	0	0	0	0	6
7	+3·0	+5·4	+3·5		−1·3		+1·4	+1·5	−1·4	−5·4	−5·8	−1·3	7
8	+5·7	+7·9	+5·1		−2·2		+2·8	+2·2	−2·8	−7·9	−8·4	−2·2	8
9	+4·2	+5·8	+3·9		−1·6		+2·1	+1·6	−2·1	−5·8	−6·3	−1·6	9
10	0	0	0		0		0	0	0	0	0	0	10
11	−1·4	−1·9	−1·2		+ ·5		− ·7	− ·5	+ ·7	+1·9	+2·1	+ ·5	11

Adding the figures in the preceding Table to those in Table B., neglecting fractions of a minute, I obtained the following.

TABLE E.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 7	− 1	+ 2	+ 7	− 1	− 7	− 5	− 1	+10	+10	0	− 6	0
1	− 5	0	+ 4	+ 4	− 2	− 7	− 2	+ 7	+ 9	+ 6	− 4	− 6	1
2	− 4	− 1	+ 6	+ 3	− 5	− 6	− 2	+10	+ 9	+ 6	− 6	− 7	2
3	− 1	+ 2	+ 2	+ 1	− 8	− 5	+ 1	+ 9	+ 8	0	− 7	− 7	3
4	+ 8	+ 5	0	− 6	− 7	− 1	+ 9	+11	+ 5	− 7	−11	− 2	4
5	+12	+ 2	− 5	−12	− 9	+ 5	+15	+ 9	− 3	−14	−10	+ 1	5
6	+16	− 2	−12	−14	− 4	+14	+18	+ 5	−10	−18	− 4	+13	6
7	+17	− 6	−19	−12	+ 6	+21	+15	− 4	−20	−19	+ 2	+15	7
8	+ 6	−19	−21	− 4	+12	+16	+ 8	− 9	−16	− 2	+10	+12	8
9	− 3	−17	− 5	+ 5	+11	+ 3	− 6	− 9	− 3	+ 7	+ 9	+ 4	9
10	− 8	− 8	+ 2	+ 6	+ 7	− 3	−11	− 5	+ 4	+14	+ 5	− 4	10
11	− 7	− 3	+ 2	+ 6	+ 4	− 7	− 8	− 2	+ 8	+10	+ 5	− 3	11

I next formed the following Table, which gives that portion of the preceding which is due to the moon's declination, the correction for the moon's declination being obtained, by means of Table A., from Table XIX.

TABLE F.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 6	+ 1	+ 7	+ 3	− 4	− 7	− 7	+ 1	+ 7	+ 5	− 3	− 7	0
1	0	+ 5	+ 5	+ 1	− 7	− 7	− 2	+ 4	+ 5	+ 1	− 6	− 7	1
2	+ 3	+ 5	+ 3	− 5	− 7	− 7	+ 1	+ 4	+ 1	− 7	− 7	− 7	2
3	+ 7	+ 7	− 1	− 6	− 6	− 4	+ 6	+ 6	0	− 6	− 6	− 4	3
4	+ 8	+ 6	− 4	− 7	− 7	+ 3	+ 8	+ 7	− 4	− 7	− 7	0	4
5	+12	− 1	− 7	− 7	− 4	+12	+12	+ 1	− 7	− 8	− 4	+12	5
6	+11	− 5	− 9	− 9	+ 2	+15	+11	− 5	− 9	− 9	+ 2	+15	6
7	− 1	−13	−13	− 6	+10	+17	+ 2	−11	−13	− 6	+10	+19	7
8	− 7	−14	− 8	+ 3	+17	+ 9	− 7	−15	−12	− 3	+15	+10	8
9	− 7	− 9	− 5	+ 8	+11	0	− 7	− 8	− 5	+ 7	+11	0	9
10	− 8	− 4	+ 3	+ 8	+ 7	− 4	− 6	− 4	0	+ 8	+ 7	− 4	10
11	− 7	− 3	+ 6	+ 7	0	− 8	− 8	− 5	+ 5	+ 6	+ 1	− 8	11

Subtracting the figures in the preceding Table from those in Table E., I get the following, which gives the correction for the calendar months.

TABLE G.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 1	− 2	− 5	+ 4	+ 3	0	+ 2	− 2	+ 3	+ 5	+ 3	+ 1	0
1	− 5	− 5	− 1	+ 3	+ 5	0	....	+ 3	+ 4	+ 5	+ 2	+ 1	1
2	− 8	− 6	+ 3	+ 8	+ 2	+ 1	− 3	+ 6	+ 8	+13	+ 1	0	2
3	− 8	− 5	+ 3	+ 7	− 2	− 1	− 5	+ 3	+ 8	+ 6	− 1	− 3	3
4	0	− 1	+ 4	+ 1	0	− 4	+	+ 4	+ 9	0	− 4	− 2	4
5	+ 2	+ 3	+ 2	− 5	− 5	− 7	+ 3	+ 8	+ 4	− 6	− 6	−11	5
6	+17	+ 3	− 3	− 5	− 6	− 1	+ 7	+10	− 1	− 9	− 6	− 2	6
7	+25	+ 7	− 6	− 6	− 4	+ 4	+13	+ 7	− 7	−13	− 8	− 4	7
8	+19	− 5	−13	− 7	− 5	+ 7	+15	+ 6	− 4	+ 1	− 5	+ 2	8
9	+ 6	− 8	0	− 3	0	+ 3	+ 1	− 1	+ 2	0	− 2	+ 4	9
10	− 2	− 4	− 5	− 2	0	+ 1	− 5	− 1	+ 4	+ 6	− 2	0	10
11	0	0	− 4	− 1	+ 4	− 1	0	+ 3	+ 2	+ 4	+ 4	+ 5	11

The numbers in the last Table are extremely irregular, and leave the correction due to the calendar months subject to much uncertainty.

The following Tables are subjoined, forming a sequel to those already printed in the Philosophical Transactions for 1831.

Table XXII. shows the height of high water at the London Docks, corresponding to the mean time of the moon's transit in each month of the year, from 6565 observations, made between the 1st of January 1808, and the 31st of December 1826 and between opposition and conjunction.

Table XXIII. is interpolated from Table XXII.

Table XXIV. gives the mean of Table XXIII. and Table V.

Table XXV. shows the time and height of high water at the London Docks, corresponding to the time of the moon's transit for every minute of horizontal parallax, from 5413 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

Tables XXVI. and XXVII. are interpolated from Table XXV.

Table XXVIII. shows the time and height of high water at the London Docks, corresponding to the time of the moon's transit, and for every three degrees of her declination, north and south, from 5424 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

Tables XXIX. and XXX. are interpolated from Table XXVIII.

Table XXXI. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every minute of the moon's horizontal parallax between opposition and conjunction.

Table XXXII. shows the difference in the height of high water, and the mean height (Column B. Table XXIII.) for every minute of the moon's horizontal parallax, between opposition and conjunction.

Table XXXIII. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every three degrees of the moon's declination, between opposition and conjunction.

Table XXXIV. shows the difference in the height of high water, and the mean height (Column B. Table XXIII.) for every three degrees of the moon's declination, between opposition and conjunction.

Table XXXV. (mean of Tables VII. and XXVI.) shows the interval between the moon's transit and the time of high water at the London Docks for every minute of her horizontal parallax, from 10,812 observations, made between the 1st of January 1808 and the 31st of December 1826.

Table XXXVI. shows the time and height of high water at the London Docks, corresponding to the mean time of the moon's transit for every minute of her horizontal parallax, from 10,812 observations, made between the 1st of January 1808 and 31st of December 1826.

Table XXXVII. is interpolated from Table XXXVI.

Table XXXVIII. (mean of Tables XI. and XXIX.) shows the interval between the moon's transit and the time of high water at the London Docks for every three degrees of her declination north or south, from 10,796 observations.

Table XXXIX. (mean of Table IX. and XXX.) shows the height of high water at the London Docks for every three degrees of the moon's declination north or south, from 10,796 observations.

Table XL. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every three degrees of her declination.

Table XLI. shows the difference in the height of high water, and the mean height (Column B. Table XXIV.) for every three degrees of the moon's declination.

Table XLII. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every minute of her horizontal parallax.

Table XLIII. shows the difference in the height of high water, and the mean height (Column B. Table XXIV.) for every minute of the moon's horizontal parallax.

Tables VI., IX., &c., given in the Philosophical Transactions for 1831, were formed from observations corresponding to transits of the moon between conjunction and opposition.

Table XXII.

Showing the Height of High Water at the London Docks corresponding to the mean time of the Moon's Transit in each month of the year; from 6565 observations made between the 1st of January 1808 and the 31st of December 1826, and between opposition and conjunction.

January.			February.			March.			April.			May.			June.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
26	22·12	23	29·2	22·42	21	24	22·61	28	16·5	22·89	22	10	22·82	21	15	22·33	20
56	22·16	21	58·8	22·66	23	53·2	22·91	22	46·3	23·12	22	40·2	23·01	23	45	22·44	20
1 26	22·62	22	1 28·3	22·9	23	1 24	23·04	25	1 14·8	23·25	22	1 11·2	22·94	22	1 14	22·63	20
1 54	22·91	21	1 59	23·18	22	1 55	23·36	27	1 44·5	23·36	21	1 41·5	23·02	20	1 45·5	22·66	23
2 26	22·58	27	2 29·5	22·94	27	2 25	23·19	20	2 14·5	23·14	21	2 10	22·91	21	2 17	22·56	22
2 57	22·73	23	3 0·2	23·11	20	2 54	22·94	25	2 44·5	22·89	23	2 41	22·83	23	2 47	22·64	20
3 26	22·54	21	3 29·5	22·66	24	3 23	22·76	22	3 16·5	22·68	21	3 10·5	22·48	20	3 15	22·3	22
3 54	22·2	24	4 1	22·47	25	3 52	22·4	21	3 47	22·39	21	3 38·2	21·15	20	3 46	22·13	25
4 26	21·41	26	4 31·2	22·05	20	4 23	22·0	26	4 15·5	20·06	19	4 9·2	21·92	25	4 16	21·79	22
4 55	21·47	25	4 59	21·96	20	4 55·5	21·33	22	4 45	21·72	22	4 41·8	21·5	26	4 45	21·4	24
5 25·5	21·06	26	5 32	21·48	23	5 24	20·94	20	5 16·2	21·15	22	5 12·2	21·09	22	5 15·5	21·2	26
5 56	20·69	25	5 59	21·16	19	5 50·5	20·56	20	5 46	20·37	20	5 40·5	20·47	23	5 46	20·64	25
6 24	20·51	23	6 26·5	20·27	18	6 23	19·87	24	6 13·2	20·04	19	6 10·5	20·11	24	6 15	20·19	22
6 56	19·96	25	6 58·8	19·82	25	6 54	19·13	20	6 44	19·46	25	6 40	19·89	26	6 45	20·08	30
7 27	19·28	23	7 29·5	19·37	17	7 23	18·8	23	7 15·5	18·96	24	7 10·5	19·45	24	7 17·5	19·79	24
7 56	19·63	23	7 59	19·2	20	7 53·5	18·81	22	7 45	19·32	21	7 40	19·69	26	7 46	19·8	21
8 26	19·61	22	8 28	19·17	19	8 23·9	18·94	23	8 14·5	19·55	24	8 11·5	19·83	27	8 14	19·98	24
8 55	19·66	20	8 59·2	19·11	20	8 54	19·18	25	8 43·5	19·72	26	8 41·8	20·34	24	8 45	20·29	25
9 24	19·94	22	9 33	20·2	19	9 24	20·08	22	9 16	20·35	23	9 11·2	20·84	26	9 17	20·37	24
9 54	20·33	21	9 57·5	20·2	19	9 53	20·33	23	9 43·5	20·86	25	9 42·5	21·15	25	9 46	21·0	20
10 26	21·12	23	10 28·8	20·78	21	10 23·5	20·44	26	10 14·5	21·39	22	10 11·2	21·54	24	10 15	21·29	22
10 57	21·44	21	11 0	20·47	23	10 55·5	21·16	27	10 45·8	21·86	30	10 40·2	21·97	21	10 47	21·55	22
11 26	21·55	21	11 31	20·68	20	11 26	21·78	23	11 17	22·55	20	11 10·8	22·09	26	11 17	21·81	21
11 55	22·03	20	12 0	21·77	21	11 54	22·34	22	11 45·8	22·56	22	11 41·2	22·38	22	11 46	21·13	20

Table XXII. (Continued).

July.			August.			September.			October.			November.			December.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
23	22·03	21	20	22·37	26	10	22·45	23	1	23·04	23	0	22·42	22	9	21·96	23
53	22·36	22	51	22·56	23	39	22·68	23	29·5	22·82	25	29	22·63	20	40	21·36	22
1 20	22·55	23	1 18·5	22·85	21	1 10	22·95	27	1 0	23·11	22	1 0	22·78	21	1 11	22·62	21
1 51	22·65	21	1 48	22·86	26	1 41	23·07	25	1 30	23·13	24	1 30	22·86	23	1 41·5	22·16	25
2 20	22·62	25	2 19·2	22·81	26	2 12	22·88	23	2 0	22·6	22	2 1	22·53	21	2 14	22·45	21
2 51	22·49	25	2 50	22·62	23	2 41	22·48	22	2 29	22·63	23	2 30	22·3	20	2 43	22·7	21
3 21	22·32	25	3 19	22·6	22	3 10	22·67	23	3 0	22·59	22	3 0	22·16	21	3 11	22·14	23
3 52	22·31	25	3 49·8	22·25	27	3 39·5	22·54	21	3 30	22·3	22	3 30	22·36	22	3 41	22·09	25
4 21	22·09	27	4 21·8	22·0	22	4 9	22·8	23	4 1	22·63	23	3 59	21·76	20	4 12	21·65	23
4 52	21·6	26	4 51·5	21·6	22	4 40	21·25	22	4 31	21·33	20	4 30	21·61	24	4 40·5	21·39	24
5 22	21·39	23	5 19	21·6	22	5 11	21·2	23	5 0·5	21·03	23	5 1	21·11	24	5 11	20·87	29
5 50	20·97	23	5 48	20·86	22	5 41	20·3	19	5 31	20·2	23	5 30	20·19	20	5 42	20·38	25
6 22	20·43	23	6 19·5	20·22	25	6 10·5	19·42	20	6 2	19·86	21	6 0	19·7	26	6 13	19·93	26
6 51	20·17	24	6 50	19·78	22	6 41	19·69	19	6 30·5	19·26	24	6 31·5	19·54	25	6 41	19·78	24
7 20	19·92	23	7 20	19·54	23	7 10·5	19·11	21	7 0	18·78	20	7 2	19·78	22	7 9·5	19·58	24
7 49	19·75	22	7 50	19·26	21	7 41·5	18·66	21	7 29·2	18·71	26	7 30	19·36	23	7 40	19·81	28
8 20	19·79	24	8 19·5	19·47	24	8 11	18·87	22	8 1	19·09	25	7 59·5	19·39	26	8 12	20·41	25
8 50	19·49	22	8 49	19·39	21	8 41	19·36	20	8 31	19·55	23	8 29	20·07	23	8 40	19·8	23
9 21	20·16	22	9 19·2	19·86	23	9 10	19·79	22	9 1	20·12	25	9 0·5	20·66	26	9 13·5	20·33	25
9 51	20·5	21	9 51	20·32	23	9 40	20·46	25	9 30	21·0	23	9 31	21·17	24	9 42·5	21·26	19
10 20	20·87	20	10 21	20·91	21	10 10	21·25	23	10 0·5	21·53	29	10 1·5	21·76	23	10 11	21·39	22
10 50	21·29	21	11 51·5	21·25	22	10 40	21·2	21	10 32	22·04	25	10 31	22·49	22	10 41·5	21·75	22
11 20	21·68	22	11 19	21·45	21	11 95	22·05	26	11 2	22·35	24	10 59	22·22	19	11 10·5	21·48	20
11 51	21·85	24	11 49	22·06	22	11 39	22·51	22	11 31	22·8	22	11 28	22·58	23	11 41	22·4	21

Table XXIII. (Interpolated from Table XXII.)

Showing the Height of High Water at the London Docks.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Moon's Transit.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	B.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·05	21·77	22·39	22·71	22·68	21·71	21·9	22·15	22·49	23·03	22·42	22·1	22·18
0 30	22·13	22·44	22·67	22·99	23·54	22·38	22·11	22·43	22·61	22·83	22·63	21·56	22·53
1 0	22·22	22·67	22·94	23·2	23·29	22·54	22·41	22·65	22·86	23·11	22·78	22·17	22·74
1 30	22·66	22·91	23·08	23·31	22·99	22·64	23·58	22·85	23·03	23·13	22·86	22·33	22·86
2 0	22·85	23·17	23·33	23·25	22·95	22·61	22·64	22·84	22·95	22·6	22·54	22·33	22·84
2 30	22·6	22·94	23·15	22·97	22·86	22·6	22·58	22·74	22·63	22·63	22·3	22·59	22·72
3 0	22·71	23·11	22·9	22·69	22·59	22·48	22·44	22·61	22·6	22·59	22·16	22·36	22·60
3 30	22·5	22·66	22·7	22·61	21·53	22·22	22·32	22·48	22·58	22·3	22·36	22·11	22·36
4 0	22·09	22·48	22·3	22·26	21·69	21·97	22·25	22·17	22·72	21·65	21·76	21·82	22·10
4 30	21·42	22·07	21·88	21·89	21·66	21·6	21·95	21·89	21·43	21·34	21·61	21·49	21·69
5 0	21·43	21·95	21·25	21·5	21·25	21·3	21·54	21·6	21·22	21·03	21·13	21·05	21·35
5 30	21·03	21·45	20·87	20·8	20·7	20·93	21·27	21·32	20·63	20·23	20·19	20·57	20·83
6 0	20·66	21·14	20·4	20·2	20·23	20·42	20·80	20·62	19·73	19·88	19·7	20·12	20·33
6 30	20·44	20·19	19·71	19·72	19·96	20·13	20·35	20·06	19·55	19·27	19·55	19·84	19·90
7 0	19·87	19·8	19·01	19·17	19·6	19·95	20·09	19·69	19·29	18·78	19·76	19·65	19·55
7 30	19·32	19·37	18·8	19·14	19·57	19·8	19·86	19·45	18·83	18·72	19·36	19·73	19·33
8 0	19·63	19·2	18·84	19·43	19·76	19·89	19·76	19·33	18·8	19·08	19·4	20·19	19·44
8 30	19·62	19·17	18·98	19·64	20·15	20·14	19·69	19·43	19·18	19·53	20·09	20·02	19·64
9 0	19·71	19·13	19·26	20·03	20·65	20·33	19·71	19·56	19·64	20·1	20·65	20·12	19·91
9 30	19·98	20·15	20·19	20·6	21·03	20·65	20·26	20·02	20·24	21·0	21·15	20·86	20·51
10 0	20·45	20·23	20·37	21·12	21·39	21·14	20·61	20·5	20·99	21·52	21·73	21·34	20·95
10 30	21·19	20·76	20·62	21·63	21·82	21·42	21·03	21·01	21·22	22·01	22·47	21·61	21·40
11 0	21·46	20·47	21·26	22·17	22·05	21·62	21·42	21·31	21·77	22·33	22·23	21·58	21·64
11 30	21·61	20·67	21·84	22·55	22·28	21·5	21·73	21·66	22·37	22·78	22·56	22·08	21·97

TABLE XXIV. (Mean of Table XXIII. and Table V.)

Moon's Transit.	Jan. Height of Tide.	Feb. Height of Tide.	March. Height of Tide.	April. Height of Tide.	May. Height of Tide.	June. Height of Tide.	July. Height of Tide.	Aug. Height of Tide.	Sept. Height of Tide.	Oct. Height of Tide.	Nov. Height of Tide.	Dec. Height of Tide.	Mean. B.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·17	22·00	22·42	22·69	22·71	22·01	21·92	22·14	22·52	23·0	22·55	22·30	22·32
0 30	22·10	22·58	22·76	22·94	23·15	22·40	22·22	22·39	22·68	22·92	22·85	21·97	22·58
1 0	22·29	22·69	22·90	23·10	23·08	22·53	22·44	22·60	22·87	23·12	22·91	22·19	22·71
1 30	22·56	22·83	23·01	23·11	22·95	22·61	22·60	22·76	22·94	23·12	22·93	22·36	22·81
2 0	22·80	22·94	23·06	23·09	22·95	22·64	22·67	22·74	22·99	23·78	22·73	22·53	22·82
2 30	22·61	22·96	22·94	22·95	22·78	22·57	22·62	22·75	22·80	22·61	22·42	22·60	22·72
3 0	22·69	22·92	22·67	22·66	22·53	22·41	22·51	22·65	22·72	22·58	22·48	22·35	22·60
3 30	22·25	22·53	22·59	22·44	21·88	22·18	22·40	22·49	22·66	22·46	22·36	22·11	22·38
4 0	22·12	22·30	22·29	22·12	21·79	22·01	22·31	22·12	22·51	21·80	21·85	21·89	22·10
4 30	21·66	21·93	21·83	21·77	21·58	21·64	22·01	21·78	21·63	21·48	21·58	21·56	21·71
5 0	21·51	21·64	21·32	21·36	21·09	21·26	21·58	21·60	21·27	20·95	21·01	21·13	21·32
5 30	21·04	21·31	20·99	20·69	20·53	20·89	21·28	21·26	20·74	20·44	20·39	20·75	20·86
6 0	20·78	20·80	20·43	20·14	20·13	20·50	20·85	20·65	20·02	19·80	19·80	20·30	20·35
6 30	20·28	19·89	19·76	19·62	19·92	20·24	20·45	20·10	19·67	19·56	19·54	19·99	19·92
7 0	19·85	19·58	19·16	19·09	19·70	19·96	20·14	19·76	19·25	19·04	19·47	19·74	19·56
7 30	19·48	19·30	19·03	19·04	19·66	19·80	19·92	19·51	18·97	18·88	19·46	19·66	19·37
8 0	19·48	19·15	18·86	19·25	19·87	19·92	19·90	19·35	18·90	19·11	19·51	19·93	19·44
8 30	19·74	19·06	19·12	19·51	20·19	20·21	19·88	19·42	19·24	19·68	19·87	20·11	19·67
9 0	19·82	19·22	19·35	20·01	20·66	20·48	19·98	19·64	19·72	20·15	20·65	20·40	20·20
9 30	20·11	20·00	20·17	20·65	21·08	20·86	20·31	20·03	20·23	21·08	20·94	22·99	20·52
10 0	20·48	20·21	20·48	21·18	21·46	21·16	20·64	20·49	20·90	21·45	21·45	21·32	20·94
10 30	21·16	20·87	20·88	21·69	21·95	21·46	21·02	20·95	21·31	22·05	22·64	21·55	21·42
11 0	21·45	20·96	21·45	22·26	22·20	21·78	21·40	21·34	21·85	22·26	22·34	21·65	21·74
11 30	21·64	20·94	22·00	22·59	22·46	21·81	21·72	21·76	22·31	22·62	22·57	22·02	21·98

TABLE XXV.

Showing the Time and the Height of High Water at the London Docks, corresponding to the Mean Time of the Moon's Transit for every minute of Horizontal Parallax; from 5413 Observations made between the 1st of January 1808 and 31st of December 1826, between opposition and conjunction.

Hor. Par. 54′. Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Hor. Par. 55′. Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Hor. Par. 56′. Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Hor. Par. 57′. Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 28·1	2 31·9	22 2·9	84	0 31·5	2 32	22 2·5	57	0 29·5	2 24·5	22 1·7	47	0 28·6	2 20·8	22 8·5	37
1 30·9	3 13·8	22 3·8	84	1 33·3	3 15·3	22 3·2	61	1 31·1	3 9·7	22 9·4	43	1 33·6	3 5·6	22 9·9	41
2 31·8	3 5·6	21 11·5	82	2 34·4	3 55·5	22 1·9	60	2 35·9	3 55·6	22 4·1	51	2 34·6	3 51·3	22 7·3	38
3 32·7	4 40·7	21 8	79	3 34·2	4 38·1	21 9·9	65	3 32·8	4 38	22 1·3	54	3 30·9	4 36	22 5·6	45
4 37	5 26·3	20 11·1	73	4 32·3	5 49	21 0·6	75	4 32·9	5 25·3	21 5·2	50	4 32·2	5 22·6	21 6·3	55
5 34·5	6 16·7	20 0	66	5 31	6 16·5	20 4·1	80	5 31	6 14·4	21 0·5	60	5 31·5	6 14·4	21 0·8	62
6 32·4	7 21·2	19 0·3	66	6 31·3	7 16·9	19 6·5	88	6 30·9	7 17·3	19 7·7	55	6 31·1	7 15·8	19 11·6	58
7 31	8 44·1	18 8·9	73	7 30·4	8 49·6	18 11·5	78	7 29·7	8 41·6	19 0·7	56	7 29·3	8 39·3	19 5	48
8 30·3	10 26·6	19 4·9	79	8 29·8	10 21·1	19 4·5	69	8 30·9	10 16·3	19 6·7	56	8 32·2	10 15·7	19 5·9	47
9 29·1	11 54·1	20 2·9	84	9 33·3	11 50·9	20 2	65	9 31·9	11 45·2	20 4·7	49	9 30·2	11 36·7	20 8·2	40
10 33·1	12 56·4	21 0·6	86	10 29·2	12 51·3	21 0·7	58	10 31·9	12 46·8	21 2·6	48	10 32·8	12 45·2	21 4·3	39
11 31·1	13 46·9	21 7·4	86	11 29·5	13 40	21 10	56	11 31	13 39·7	21 11	43	11 31·5	13 36	22 0	39

Table XXV. (Continued).

Hor. Par. 58′.				Hor. Par. 59′.				Hor. Par. 60′.				Hor. Par. 61′.			
Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
32·4	2 20	22 6·9	35	0 32	2 15	22 4·8	42	0 32·2	2 13·2	22 11·3	49	0 30·4	2 5·4	23 2·7	102
1 28·2	2 59·5	23 1·1	37	1 33·8	3 1·6	23 4·8	43	1 33·6	2 58·6	23 4·2	51	1 31·3	2 53·1	23 4·7	92
2 28·3	3 49·7	23 0·9	41	2 35·1	3 44·4	23 3·7	48	2 34·6	3 47·1	23 3·2	79	2 25·7	3 34·5	23 6·4	51
3 34·5	4 33·8	22 6·6	47	3 30·9	4 28·3	23 0	63	3 29·8	4 25·8	23 2·3	97				
4 29·8	5 20·3	21 11·7	57	4 33	5 20·3	22 5	103	4 26	5 10·9	22 8·1	38				
5 30·8	6 12	20 11·7	62	5 28·6	6 7·9	21 7·6	121	5 15·3	5 22·3	21 10·3	3				
6 31·8	7 12·4	20 2·4	64	6 30·8	7 10·8	20 5·5	121								
7 29·5	8 30·5	19 8	64	7 27·8	8 22·1	19 9·1	103	7 41·2	8 42·8	19 11·7	25				
8 25·8	10 2·7	19 10·5	54	8 25·6	9 54·7	19 10·6	67	8 35·8	10 5·4	20 0·5	77				
9 31·3	11 32·2	20 10	48	9 30·5	11 23·7	21 0·5	53	9 27·2	11 15·6	21 1·4	84	9 43·9	11 36·1	21 0·8	31
10 32·4	12 40·2	21 5·6	44	10 30·7	12 31·9	21 8·3	40	10 31·3	12 26·8	21 9·2	57	10 32·9	12 27·3	21 10	79
11 31·2	13 30·9	22 4·7	43	11 29·1	13 26·4	22 6·5	39	11 28·1	13 18·7	22 4·9	46	11 31·2	13 18·5	22 5·7	98

Table XXVI. (Interpolated from Table XXV.)

Showing the Interval between the Moon's Transit, A.M., and the Time of High Water at the London Docks, for every minute of her Horizontal Parallax between opposition and conjunction.

Moon's Transit. A.M.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 10	2 6	2 3	1 58·8	1 55	1 51	1 46·7	1 41·5
0 30	2 3·2	2 0·7	1 54·9	1 51·8	1 48·2	1 43·5	1 41·5	1 35
1 0	1 53	1 51·8	1 46·9	1 41·8	1 39·5	1 36·2	1 33·6	1 28·5
1 30	1 42·8	1 43	1 38·9	1 31·8	1 30·9	1 28·9	1 26	1 22
2 0	1 33·9	1 32·8	1 30·6	1 24·5	1 25·9	1 19·7	1 19·8	1 16
2 30	1 25	1 22·6	1 21·4	1 17·7	1 20·9	1 10·6	1 13·7	1 10
3 0	1 16·8	1 13·9	1 13·5	1 11·5	1 10·7	1 4·1	1 4·8	
3 30	1 8·7	1 5·1	1 5·9	1 5·3	1 0·5	0 57·6	0 56	
4 0	1 0·5	0 59·7	0 59·4	0 58	0 55·4	0 52·8	0 50	
4 30	0 52·3	0 54·4	0 53	0 50·8	0 50·3	0 48	0 44	
5 0	0 46·3	0 49·6	0 48	0 46·5	0 45·8	0 43·6		
5 30	0 42·8	0 45·7	0 43·6	0 43	0 41·3	0 39·2		
6 0	0 44	0 43	0 42	0 41	0 39	0 37·8		
6 30	0 51	0 45·6	0 46·3	0 44·5	0 40·3	0 39·9		
7 0	1 0	0 55·6	0 56·8	0 53·9	0 48	0 46		
7 30	1 13·5	1 12·3	1 12	1 3·3	1 1·2	0 55·2		
8 0	1 34	1 31	1 28	1 22	1 20	1 11·5		
8 30	1 56·3	1 51·3	1 44·9	1 42·4	1 38·8	1 31·2	1 27	
9 0	2 12·6	2 7·5	2 1·5	1 57	1 52	1 44·5	1 40·5	
9 30	2 21·2	2 17·6	2 12·4	2 6·5	2 0·7	1 53	1 49	
10 0	2 24·6	2 22·5	2 16	2 12	2 6	1 59	1 53·5	1 54
10 30	2 24·2	2 22·2	2 14·8	2 12·5	2 7·8	2 1·2	1 55·5	1 54·8
11 0	2 21	2 17·5	2 13	2 10	2 5·5	2 1	1 54	1 52
11 30	2 15·9	2 10·4	2 8·6	2 4·8	2 0	1 57·2	1 50·5	1 47

Table XXVII. (Interpolated from Table XXV.)

Showing the Height of High Water at the London Docks for every minute of the Moon's Horizontal Parallax between opposition and conjunction.

Moon's Transit. A.M.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	21·86	22·05	22·06	22·39	22·48	22·48	22·67	22·85
0 30	22·30	22·28	22·14	22·70	22·56	22·39	22·93	23·22
1 0	22·40	22·36	22·48	22·76	22·83	22·85	23·14	23·25
1 30	22·30	22·24	22·78	22·81	23·08	23·33	23·33	23·47
2 0	22·20	22·22	22·58	22·73	23·07	23·35	23·31	23·47
2 30	21·96	22·19	22·37	22·62	23·05	23·30	23·25	23·54
3 0	21·84	22·04	22·24	22·54	22·81	23·15	23·22	
3 30	21·67	21·86	22·11	22·46	22·58	23·01	23·18	
4 0	21·36	21·44	21·81	22·02	22·24	22·72	22·92	
4 30	21·03	21·06	21·47	21·56	21·96	22·44	21·64	
5 0	20·56	20·73	21·24	21·30	21·48	21·96		
5 30	20·08	20·35	21·04	21·06	20·98	21·61		
6 0	19·58	19·96	20·36	20·53	20·59	21·04		
6 30	19·10	19·55	19·66	19·99	20·22	20·49		
7 0	18·73	19·18	19·36	19·69	19·95	20·11		
7 30	17·72	18·96	19·08	19·42	19·66	19·76		
8 0	19·00	19·12	19·31	19·45	19·76	19·72		
8 30	19·41	19·38	19·55	19·48	19·92	19·95	20·03	
9 0	19·82	19·73	19·96	20·05	20·37	20·44	20·55	
9 30	20·22	20·14	20·36	20·68	20·82	21·04	21·14	
10 0	20·62	20·60	20·78	21·02	21·13	21·36	21·44	21·31
10 30	21·03	21·06	21·24	21·33	21·45	21·68	21·76	21·79
11 0	21·33	21·44	21·55	21·66	21·91	22·21	22·09	22·13
11 30	21·60	21·83	21·91	21·99	22·37	22·53	22·42	22·55

Table XXVIII.

Showing the Time and Height of High Water at the London Docks, corresponding to the mean time of the Moon's Transit for every three degrees of her declination north and south; from 5424 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

1° 30′ S. to 1° 30′ N. Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.	
0 35·5	2 29	22 7·6	19
1 38·1	3 16·2	22 11·8	20
2 41·3	4 2·6	22 6·3	21
3 36·3	4 45·8	22 6·1	19
4 36·9	5 40	21 4·2	18
5 33·8	6 31·3	20 10·3	16
6 31·8	7 31·7	19 7·4	14
7 23·3	8 45·9	19 9·4	16
8 22·1	10 16·6	20 0·6	18
9 28·8	11 35	20 8·3	17
10 33·2	12 52·7	21 10·4	20
11 30·8	13 41	22 1	20

Table XXVIII. (Continued).

1° 30′ to 4° 30′ North Decl.				4° 30′ to 7° 30′ North Decl.				7° 30′ to 10° 30′ North Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 32·4	2 27·6	22 8·7	19	0 32	2 24·7	23 0	22	0 33·9	2 22·3	22 10·6	15
1 34·5	3 11·6	23 0·9	18	1 38	3 12·7	23 3·2	24	1 34·7	3 13·7	22 10·6	13
2 41·6	4 6	22 11	14	2 37·5	4 0·2	22 5·2	21	2 38	4 1·1	22 8·8	22
3 42	4 47·3	22 5·2	17	3 35·7	4 48·7	22 3·7	19	3 38	4 48·1	22 3·7	19
4 38·9	5 37·9	21 10	17	4 29·8	5 37·6	21 11·8	19	4 37·2	5 39·4	21 7·5	19
5 32·1	6 28·5	20 11	17	5 32·3	6 23	20 10·9	16	5 31·4	6 30·2	20 5·6	21
6 27·8	7 32	19 9·9	20	6 31·8	7 28·5	20 6·6	20	6 26·1	7 30·2	19 6·2	20
7 25·8	8 46·3	19 10·9	19	7 29·2	8 45·5	19 5·2	19	7 25	8 48·3	19 9·2	18
8 26·8	10 21	20 2·7	19	8 26·4	10 16·6	20 0	18	8 23·7	10 13·7	19 11·4	20
9 23·4	11 36	20 8·9	20	9 27·1	11 44·2	21 1·1	20	9 23·4	11 42·6	20 8·8	21
10 28·1	12 49·4	21 10·8	21	10 25·6	12 43·7	21 11	21	10 32·4	12 50·3	22 0·7	17
11 28·9	13 43·6	22 3·5	18	11 27	13 37·6	22 4·8	17	11 30	13 39	22 3·7	20
1° 30′ to 4° 30′ South Decl.				4° 30′ to 7° 30′ South Decl.				7° 30′ to 10° 30′ South Decl.			
0 30·5	2 27·8	22 8·1	18	0 33·2	2 27·8	22 10·4	21	0 34·8	2 32·5	22 8	22
1 35	3 12·4	22 11·7	19	1 37·6	3 17·5	23 2·8	16	1 36·9	3 14·2	22 10·8	19
2 39·2	4 1·8	23 0·6	19	2 45·3	4 6·5	22 10·9	19	2 36·4	4 1·4	22 10·4	24
3 35·5	4 45·5	22 8·3	20	3 44·2	4 52·5	22 8·5	18	3 34·3	4 43·3	22 5·5	18
4 41·7	5 41·3	21 5·8	15	4 33·8	5 37·5	21 4·8	16	4 42·5	5 40·8	21 9·2	18
5 36·3	6 31·7	20 11·5	17	5 34·3	6 29·4	20 9·8	18	4 35	6 38·2	20 10·1	17
6 23·2	7 20·8	20 3·8	17	6 31·9	7 31·9	20 3	21	6 28·3	7 24·5	20 4·6	19
7 27·8	8 45·5	19 10·3	18	7 21·6	8 35	19 3·1	18	7 28	8 49·1	19 6·7	18
8 29·9	10 20·3	20 5·4	17	8 25·3	10 14·5	19 11·6	20	8 33·1	10 24·5	19 7·4	20
9 28·2	11 43·5	20 10·9	17	9 25·1	11 39·7	20 9·7	19	9 31·2	11 41·5	20 10·1	20
10 27·5	12 46·5	21 8·4	17	10 33·3	12 49	21 6·8	18	10 23·5	12 46·7	21 8	21
11 28·1	13 40·8	22 5·3	19	11 32·7	13 44·3	22 4	16	11 30	13 40·4	22 3·4	22

10° 30′ to 13° 30′ North Decl.				13° 30′ to 16° 30′ North Decl.				16° 30′ to 19° 30′ North Decl.			
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 28·6	2 23·8	22 3·3	22	0 27·4	2 20	22 7·1	28	0 26·4	2 17·6	22 5·6	36
1 33·8	3 8	23 2·7	25	1 26·9	3 0·1	22 9·8	28	1 28·2	3 5·8	22 8·6	37
2 35·4	3 54·4	22 10	18	2 28·4	3 49	22 1·9	27	2 24	3 41·5	22 7·3	38
3 36	4 41·5	22 7·1	20	3 30·1	4 29·4	22 6·4	26	3 28	4 29·5	22 5·4	42
4 31·2	5 32·5	21 6	24	4 30·5	5 22·3	21 8·9	26	4 25·3	5 18·7	21 7·2	34
5 28·6	6 18·2	21 1·8	22	5 28·3	6 14·2	20 8·7	26	5 25·1	6 5	21 0	38
6 29·5	7 16·1	19 11·6	17	6 31·7	7 21·9	19 7·3	26	6 28·9	7 12·6	19 9·2	40
7 24	8 35·2	19 4·5	21	7 24·2	8 38·2	19 9·9	25	7 24·8	8 29·7	19 6	38
8 27·7	10 18·1	19 10	23	8 26·8	10 15·9	19 7·7	28	8 31·2	10 9·8	19 8·5	37
9 24·1	11 35·2	20 10·6	22	9 31·9	11 38·7	20 7·2	24	9 31	11 37·1	20 5·8	37
10 29·2	12 47·3	21 7·1	26	10 31·7	12 47	21 4·4	27	10 31·4	12 42·5	21 5·1	34
11 24·7	13 31·6	22 4·6	25	11 31·6	13 35	22 1·6	28	11 32·3	13 27·8	22 0·1	38
10° 30′ to 13° 30′ South Decl.				13° 30′ to 16° 30′ South Decl.				16° 30′ to 19° 30′ South Decl.			
0 33	2 27·4	22 9·2	25	0 32	2 15·8	22 9·6	21	0 32·7	2 16·3	22 7·5	34
1 30·5	3 10·4	22 11·1	23	1 33·2	3 7·2	23 2·2	29	1 35·2	3 2·8	23 1·9	29
2 35·4	3 53·7	22 6·4	24	2 37·5	3 55	22 10	24	2 31·3	3 48·7	22 11·6	37
3 37·3	4 41·8	22 4·8	25	3 41·6	4 43	22 6·7	20	3 31·4	4 31	22 6·8	43
4 38	5 28·8	21 7·2	27	4 37·7	5 24·8	21 9·8	26	4 34·4	5 19·6	21 9·8	39
5 35·5	6 25·7	21 1·2	20	5 35·4	6 15·3	21 2·3	33	5 35·6	6 14·1	20 10·5	36
6 34·4	7 31·6	20 0·1	25	6 35·6	7 22·9	19 8	24	6 38·1	7 19·3	20 0·5	39
7 31·2	8 41·8	19 1·9	21	7 30·3	8 35·5	19 5·2	26	7 37·7	8 40·2	19 2·1	40
8 29·7	10 14·1	20 0·6	22	8 29·9	10 10·9	19 7·1	22	8 38·8	10 10·6	19 7·8	40
9 32·9	11 39·3	20 4·5	22	9 31·4	11 32·5	20 9·5	33	9 36·5	11 36·1	20 4·7	26
10 31	12 41·4	21 10·9	22	10 32·2	12 41·3	21 10·1	24	10 33·6	12 37·1	21 5·3	38
11 29	13 30·6	22 6	24	11 29·8	13 27·7	21 10·9	37	11 31·1	13 27·8	22 0·1	37

Table XXVIII. (Continued).

19° 30′ to 22° 30′ North Decl.				22° 30′ to 25° 30′ North Decl.				Above 25° 30′ North Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 26·2	2 10·4	22 6·5	28	0 28·9	2 7·9	22 1	31	0 25·2	2 6·8	22 3·4	14
1 26	2 49	23 1·3	31	1 24·3	2 52·3	22 7·1	17	1 23·1	2 50·2	22 3	21
2 24·5	3 35	22 7·6	27	2 22·7	3 35·5	22 5·6	26	2 23·9	3 29·2	22 6	14
3 26·1	4 23	22 0·9	25	3 27·1	4 16	22 4·1	20	3 16·4	4 7·9	22 3·4	24
4 23·7	5 9·8	21 10·7	34	4 23·6	5 1·2	21 6	24	4 13·2	4 49·5	21 5·5	21
5 23·2	5 56·2	20 10·1	36	5 28·7	5 58·6	20 5·3	19	5 22	5 44·5	20 7	23
6 30·7	7 3·9	20 0	27	6 21·8	6 42·3	19 3·3	51	6 24·5	6 52·4	19 8	25
7 27·8	8 19·8	18 10·2	31	7 38·2	8 26·7	18 11·1	23	7 28·5	8 14·8	19 0	23
8 33·1	10 11·5	19 6·7	29	8 30·5	10 31	19 4·5	24	8 29·7	9 55·9	19 4·3	22
9 29·2	11 34·2	20 10·7	31	9 36·1	11 33·2	20 1·8	23	9 34·1	11 24·8	19 11·5	21
10 35·1	12 40·2	21 2·2	30	10 31·2	12 31·6	21 1·1	22	10 33·4	12 32·4	20 10·5	21
11 31	13 31·3	21 10·9	29	11 35·8	13 27	22 0·7	25	11 30·2	13 23·1	21 10·3	18

19° 30′ to 22° 30′ South Decl.				22° 30′ to 25° 30′ South Decl.				Above 25° 30′ South Decl.			
0 30·3	2 19	22 7·5	32	0 34·9	2 19·4	22 3·9	25	0 32·9	2 11	22 4·9	20
1 33·6	3 2	22 11·8	30	1 35·2	3 3·9	22 8·1	19	1 30	2 54·3	22 6·1	22
2 32·7	3 46·4	22 10·9	28	2 33·4	3 44·8	22 11·6	28	2 28·6	3 34·5	22 6·4	20
3 30·9	4 30·3	22 3·1	32	3 32·5	4 27	22 4	22	3 31·7	4 20·9	22 4·5	21
4 39·3	5 25·8	21 11·3	23	4 32·9	5 18·8	21 6·2	25	4 32·6	5 6·7	21 9	23
5 34·1	6 9·8	20 9·7	28	5 34·8	6 9·4	20 10·6	25	5 29·8	5 51·8	20 7·9	24
6 35·3	7 14	19 4·9	26	6 37	7 3·4	19 7·6	23	6 35·2	6 53·2	19 5·1	25
7 40·1	8 43·2	18 11·2	29	7 40·7	8 32	19 1·7	24	7 38·2	8 9·5	19 0·1	21
8 33·6	10 12·5	19 4·8	31	8 37·7	10 6·4	19 2·5	24	8 43·4	16 6·9	18 9·3	18
9 39·4	11 46·4	20 6·9	28	9 37·7	11 31·8	20 1·6	24	9 36	11 24·5	19 11·4	21
10 34·8	12 40·8	21 4·6	33	10 33·7	12 30·8	20 11·1	18	10 37·5	12 30·7	21 1·5	20
11 30·2	13 26·6	21 11·3	27	11 35·2	13 30·4	22 1·6	23	11 30·5	13 19·4	21 11·3	18

Table XXIX. (Interpolated from Table XXVIII.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks, for every three degrees of her declination north or south, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	1 54·9	1 57	1 54·4	1 58·8	1 55	1 48·2	1 47·3	1 46	1 49·1	1 40·5	1 50·4
1 30	1 40	1 38·7	1 39·2	1 39·6	1 37·6	1 33·6	1 33·6	1 25·7	1 28·3	1 24·2	1 34
2 30	1 24·3	1 25·6	1 24·6	1 25·6	1 20·2	1 19·8	1 15·8	1 11·8	1 11·5	1 4·2	1 18·3
3 30	1 10·8	1 10	1 12·4	1 11	1 6·5	1 2·2	1 0·5	0 57·4	0 51·7	0 48·8	1 3
4 30	1 4	1 0·7	1 1	1 1·2	0 56·6	0 50·2	0 49·3	0 47	0 41·4	0 33·7	52·3
5 30	0 57·9	0 56·2	0 53·2	0 56·2	0 50·1	0 43·2	0 39·2	0 34·5	0 32·4	0 22·3	44·5
6 30	0 59·8	1 1·6	0 58·4	0 55·6	0 51·8	0 48·5	0 41·8	0 35·8	0 23·6	0 23	46
7 30	1 26	1 20·4	1 16·9	1 19	1 12·1	1 10·8	1 3·5	0 55·5	0 45·2	0 38	1 6·7
8 30	1 57·3	1 54	1 51·8	1 51·5	1 48	1 45·7	1 33	1 30·7	1 28·3	1 21·3	1 42·8
9 30	2 7	2 14·5	2 16·3	2 15·3	2 9·1	2 3·3	2 1·8	2 4·1	1 53·5	1 48·3	2 5·3
10 30	2 18·9	2 16·5	2 16·9	2 20·4	2 14·2	2 12	2 7	2 4·7	1 58·7	1 55·5	2 10·5
11 30	2 10	2 12·7	2 11·1	2 9·7	2 3·7	2 0·7	1 56·4	1 58·5	1 54·5	1 50·9	2 3

Table XXX. (Interpolated from Table XXVIII.)

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north and south, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	22·58	22·69	22·90	22·78	22·52	22·38	22·54	22·57	22·20	22·31	22·58
1 30	22·94	22·99	23·21	22·87	23·04	23·07	22·94	23·04	22·62	22·38	22·91
2 30	22·61	22·98	22·76	22·80	22·69	22·52	22·77	22·75	22·70	22·51	22·71
3 30	22·51	22·63	22·52	22·42	22·51	22·54	22·50	22·15	22·33	22·30	22·44
4 30	21·50	21·80	21·70	21·80	21·62	21·82	21·70	21·93	21·48	21·50	21·69
5 30	20·90	20·98	20·89	20·70	21·13	21·02	20·93	20·80	20·68	20·56	20·86
6 30	19·65	20·05	20·41	19·94	19·63	19·71	19·96	19·75	19·44	19·54	19·81
7 30	19·81	19·90	19·40	19·67	19·55	19·62	19·34	18·94	19·06	19·03	19·43
8 30	20·14	20·34	20·03	19·80	19·96	19·64	19·64	19·45	19·27	19·08	19·74
9 30	20·71	20·88	20·99	20·84	20·65	20·67	20·38	20·64	20·04	19·88	20·57
10 0	21·81	21·81	21·73	21·88	21·75	21·58	21·40	21·25	20·97	20·91	21·51
11 30	22·07	22·37	22·36	22·29	22·44	22·00	22.00	21·92	22·09	21·90	22·14

Table XXXI.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (A.M. part of Table II.) for every minute of the Moon's Horizontal Parallax, between opposition and conjunction.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	+ 3	+11	+ 5	+ 2	− 2	− 6	− 8	−15
1 30	+ 8	+ 8	+ 4	+ 3	− 4	− 6	− 9	−13
2 30	+ 7	+ 5	+ 3	0	+ 3	− 7	− 4	− 8
3 30	+ 7	+ 3	+ 4	+ 3	− 1	− 4	− 6	
4 30	0	+ 2	+ 1	− 1	− 2	− 4	− 8	
5 30	+ 1	+ 4	+ 2	+ 1	− 1	− 3		
6 30	+ 7	+ 2	+ 2	+ 1	− 3	− 4		
7 30	+ 9	+ 7	+ 7	− 2	− 4	−10		
8 30	+16	+11	+ 5	+ 2	− 1	− 9	−13	
9 30	+16	+13	+ 7	+ 2	− 4	−12	−16	
10 30	+14	+12	+ 5	+ 3	− 2	− 9	−14	−15
11 30	+12	+ 6	+ 5	+ 1	− 4	− 7	−13	−17

Table XXXII.

Showing the Difference in the Height of High Water, and the Mean Height (Column B. Table XXIII.) for every minute of the Moon's Horizontal Parallax, and between opposition and conjunction.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	−·23	−·25	−·39	+·17	+·03		+1·07	+·69
1 30	−·56	−·62	−·08	−·05	+·22	+·47	+ ·61	+·61
2 30	−·74	−·53	−·35	−·10	+·33	+·58	+ ·53	+·82
3 30	−·69	−·50	−·25	+·10	+·22	+·65	+ ·82	
4 30	−·66	−·63	−·22	−·13	+·27	+·75		
5 30	−·75	−·48	+·21	+·23	+·15	+·78		
6 30	−·80	−·35	−·24	+·09	+·32	+·59		
7 30	−·61	−·37	−·25	+·09	+·33	+·43		
8 30	−·23	−·26	−·09	−·16	+·28	+·31	+ ·39	
9 30	−·29	−·37	−·15	+·17	+·31	+·53	+ ·63	
10 30	−·37	−·34	−·16	−·07	+·05	+·28	+ ·36	
11 30	−·37	−·14	−·06	+·02	+·40	+·56	+ ·45	

Table XXXIII.

Showing the Difference in the Interval between the Time of Moon's Transit and the Time of High Water, and the Mean Interval (A.M. part of Table II.) for every three degrees of the Moon's Declination, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 30	+ 5	+ 7	+ 4	+ 9	+ 5	− 2	− 3	− 4	− 8	− 9
1 30	+ 5	+ 4	+ 4	+ 5	+ 3	− 1	− 1	− 9	− 7	−11
2 30	+ 6	+ 8	+ 7	+ 8	+ 2	+ 2	− 2	− 6	− 6	−14
3 30	+ 9	+ 8	+10	+ 9	+ 5	0	− 1	− 5	−10	−13
4 30	+12	+ 9	+ 9	+ 9	+ 5	− 2	− 3	− 5	−11	−18
5 30	+16	+14	+11	+14	+ 8	+ 1	− 3	− 7	−10	−20
6 30	+16	+18	+14	+12	+ 8	+ 5	− 2	− 8	−20	−21
7 30	+21	+15	+12	+14	+ 7	+ 6	− 1	− 9	−20	−27
8 30	+17	+14	+12	+12	+ 8	+ 8	− 7	− 3	−12	−19
9 30	+ 2	+10	+11	+10	+ 4	− 2	− 3	− 1	−11	−17
10 30	+ 9	+ 7	+ 7	+10	+ 4	+ 2	− 3	− 5	−11	−14
11 30	+ 6	+ 9	+ 7	+ 6	0	− 3	− 8	− 5	− 9	−13

Table XXXIV.

Showing the Difference in the Height of High Water, and the Mean Height (Column B. Table XXIII.) for every three degrees of the Moon's Declination, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	+·03	+·16	+·37	+·25	−·01	+·15	+·01	+·04	−·33	−·22
1 30	+·08	+·13	+·35	+·01	+·18	+·21	+·08	+·18	−·24	−·18
2 30	−·11	+·26	+·04	+·08	−·03	−·20	+·05	+·03	−·02	−·21
3 30	+·15	+·27	+·16	+·06	+·15	+·18	+·14	−·21	−·03	−·06
4 30	−·19	+·12	+·01	+·11	−·07	+·13	+·01	+·24	−·21	−·19
5 30	+·07	+·15	+·06	−·13	+·30	+·19	+·10	−·03	−·15	−·27
6 30	−·25	+·15	+·51	+·04	−·27	−·19	+·06	−·15	−·46	−·36
7 30	+·48	+·57	+·07	+·34	+·22	+·20	+·01	−·39	−·27	−·30
8 30	+·50	+·70	+·39	+·16	+·32	0	0	−·19	−·37	−·58
9 30	+·20	+·37	+·48	+·33	+·14	+·16	−·13	+·13	−·47	−·63
10 30	+·41	+·41	+·33	+·48	+·35	+·18	0	−·15	−·43	−·49
11 30	+·10	+·40	+·39	+·32	+·47	+·03	+·03	−·05	+·12	−·07

Table XXXV. (Mean of Tables VII. and XXVI.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks for every minute of her Horizontal Parallax; from 10,812 observations, made between the 1st of January 1808 and the 31st of December 1826.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 9·7	2 5·8	2 1·5	1 57·6	1 54·6	1 52	1 45	1 42
0 30	2 2·5	1 59·9	1 53·2	1 51·9	1 47·6	1 44·2	1 41·3	1 36·8
1 0	1 52·8	1 50·7	1 46	1 44·5	1 40·1	1 37·6	1 33·5	1 30
1 30	1 43·2	1 41·3	1 38·2	1 35·9	1 32·5	1 30·7	1 25·7	1 23·2
2 0	1 34	1 32·5	1 29·6	1 27	1 26·5	1 22·3	1 19·2	1 16·5
2 30	1 25	1 22·8	1 20·5	1 18·3	1 20·3	1 13·3	1 13	1 10·3
3 0	1 16·9	1 14·3	1 12·9	1 11	1 11·6	1 6·2	1 5	
3 30	1 8·8	1 6	1 6	1 4·6	1 3·4	0 59	0 56·7	
4 0	1 1·1	0 59·5	0 59	0 56·8	0 56·7	0 53·4	0 50	
4 30	0 51·5	0 53·4	0 53·6	0 50·2	0 50·2	0 48·3	0 43·8	
5 0	0 46·3	0 48·1	0 47·8	0 46·2	0 45·4	0 43·8		
5 30	0 43·4	0 44·2	0 42·8	0 43·7	0 41·3	0 40·5		
6 0	0 43·5	0 43·2	0 40·7	0 42·2	0 39·4	0 39·1		
6 30	0 48·6	0 46·7	0 43·6	0 44	0 40·6	0 40·5		
7 0	0 58·1	0 54·6	0 52·9	0 51·2	0 47·5	0 46·5		
7 30	1 14	1 7·7	1 7·6	1 2·4	0 59·6	0 57		
8 0	1 36·1	1 26·5	1 25	1 20·4	1 17·4	1 14	1 11·6	
8 30	1 58·4	1 49·1	1 43·8	1 41·4	1 35·4	1 31·6	1 27·3	
9 0	2 13·6	2 6·1	2 0	1 56·8	1 49·2	1 44·8	1 39·8	
9 30	2 21·7	2 16·6	2 12·4	2 6·4	1 58·9	1 53·2	1 48·2	
10 0	2 25·3	2 22	2 16·7	2 11	2 3·4	1 58·5	1 53·3	
10 30	2 24·8	22·7	2 16·5	2 11	2 7·8	2 0·6	1 55·7	1 54
11 0	2 21·4	2 18·7	1 14·2	2 8·1	2 5·7	2 0·7	1 54·1	1 51
11 30	2 16	2 11·6	2 9	2 3·3	2 0·4	1 58	1 48·7	1 46·9

Table XXXVI.

Showing the Time and Height of High Water at the London Docks, corresponding to the mean time of Moon's Transit for every minute of her Horizontal Parallax; from 10,812 observations, made between the 1st of January 1808 and 31st of December 1826.

Hor. Par. 54′.			Hor. Par. 55′.			Hor. Par. 56′.			Hor. Par. 57′.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
0 31·8	22·15	179	0 31·5	22·24	108	0 31·5	22·24	85	0 31·9	22·67	80
1 25·2	22·27	173	1 27·7	22·32	115	1 26·4	22·80	93	1 30	22·76	79
2 33·5	22·04	162	2 36	22·23	131	2 34·2	22·38	89	2 34	22·73	83
3 27·5	21·69	152	3 30	21·97	130	3 27·8	22·15	110	3 25·8	22·45	91
4 33·2	21·00	147	4 32	21·11	149	4 33·4	21·42	100	4 32·6	21·66	114
5 34·5	20·09	121	5 32	20·30	170	5 31	20·73	118	5 32·7	21·05	119
6 33·6	19·15	134	6 34·3	19·54	159	6 34	19·67	121	6 32·4	19·91	115
7 28·4	18·73	145	7 28	19·00	151	7 28	19·12	114	7 26·9	19·45	104
8 32·4	19·36	165	8 33·3	19·50	123	8 33·1	19·63	121	8 33	19·67	91
9 35·3	20·21	166	9 35·9	20·31	131	9 32·5	20·43	91	9 33	20·75	84
10 35	21·14	170	10 34·2	21·06	114	10 33·8	21·28	98	10 34	21·08	79
11 33·8	21·64	169	11 32·6	21·85	116	11 32·6	21·87	78	11 35	22·19	82

Hor. Par. 58′.			Hor. Par. 59′.			Hor. Par. 60′.			Hor. Par. 61′.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
0 34·9	22·77	68	0 34·5	22·66	75	0 32	22·93	101	0 31	23·18	194
1 27·7	23·08	74	1 33·1	23·13	85	1 32	23·25	117	1 29·3	23·41	170
2 30·6	22·93	91	2 32·6	23·13	96	2 34·3	23·35	151	2 25·6	23·55	94
3 29·3	22·50	93	3 27·4	22·93	136	3 29	23·24	187			
4 31	22·09	112	4 32·7	22·36	217	4 25·1	22·58	65			
5 31·3	20·94	136	5 30	21·60	240						
6 31·2	20·23	121	6 32·4	20·45	242						
7 29	19·65	128	7 29·1	19·85	191	7 38·3	20·03	65			
8 30·7	19·75	103	8 28·6	20·04	123	8 35·2	20·18	176			
9 35·5	20·73	94	9 33	20·78	104	9 31·2	21·09	182	9 40·8	21·21	53
10 32·8	21·57	87	10 34	21·74	82	10 32·6	21·85	120	10 32·6	21·85	152
11 35	22·31	82	11 30·4	22·52	82	11 32·2	22·47	96	11 33	22·61	195

Table XXXVII. (Interpolated from Table XXXVI.)

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	22·18	22·23	22·23	22·65	22·73	22·85	22·91	23·17
1 30	22·26	22·32	22·78	22·76	23·07	23·12	23·26	23·41
2 30	22·06	22·24	22·40	22·73	22·92	23·13	23·34	23·54
3 30	21·67	21·97	22·13	22·42	22·50	22·91	23·23	
4 30	20·97	21·14	21·49	21·69	22·10	22·39	22·51	
5 30	20·14	20·33	20·74	21·08	20·96	21·60		
6 30	19·19	19·59	19·72	19·94	20·24	20·48		
7 30	18·74	19·02	19·12	19·46	19·65	19·86	20·04	
8 30	19·33	19·47	19·60	19·64	19·75	20·05	20·17	
9 30	20·14	20·23	20·40	20·71	20·69	20·74	21·07	21·11
10 30	21·12	21·02	21·22	21·33	21·53	21·69	21·82	21·82
11 30	21·61	21·81	21·84	22·12	22·26	22·52	22·45	22·57

Table XXXVIII. (Mean of Tables XI. and XXIX.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks for every three degrees of her Declination north or south; from 10,796 observations.

Moon's Transit. A.M.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 3·5	2 4	2 3	2 2	1 59	1 56	1 54	1 51	1 48	1 46·5	1 56·7
0 30	1 56	1 56·7	1 56·5	1 55·6	1 52	1 49·3	1 48	1 45·5	1 42·4	1 39·6	1 50·2
1 0	1 48	1 47	1 48·5	1 47	1 45	1 43·5	1 39	1 36	1 35	1 32	1 42·1
1 30	1 40·4	1 36	1 40	1 38·5	1 38·8	1 36·2	1 29	1 27	1 28·3	1 24	1 33·8
2 0	1 32	1 28·5	1 32	1 32	1 30	1 29·5	1 23	1 20	1 21	1 16·5	1 26·5
2 30	1 24	1 22·5	1 24·4	1 26·3	1 20·6	1 19·8	1 16·8	1 12·7	1 12·2	1 7·6	1 18·7
3 0	1 18	1 17	1 17·5	1 18	1 13·5	1 12	1 9	1 5	1 4·5	0 59	1 11·3
3 30	1 12·4	1 12	1 11	1 10·1	1 6·8	1 2·8	1 0·8	0 58·4	0 56	0 50	1 4
4 0	1 7	1 6	1 5	1 4·5	1 2	0 56	0 54	0 51	0 49	0 41	0 57
4 30	1 1·7	1 0·4	1 0	1 0·3	0 58·1	0 50·6	0 47·5	0 45·5	0 40·5	0 34	0 52
5 0	1 0	0 57	0 57	0 57·0	0 54	0 46·5	0 43·5	0 40·5	0 35	0 29	0 48
5 30	0 59·4	0 56	0 54·6	0 55·7	0 49·6	0 43·3	0 39·8	0 36·1	0 32	0 25·1	0 45·4
6 0	1 0	0 57·5	0 55	0 56	0 50·5	0 44	0 39	0 34·5	0 28	0 22	0 44·7
6 30	1 3·5	1 2·7	0 58	0 57·1	0 52·7	0 46·9	0 40·2	0 35·3	0 25·7	0 23	0 46·5
7 0	1 11	1 11	1 7	1 5	1 1	0 54	0 48	0 43·5	0 34	0 26	0 54
7 30	1 23	1 21·8	1 19·5	1 16	1 14	1 4·4	1 0·4	0 56·9	0 47	0 35·3	1 5·8
8 0	1 40	1 39	1 36	1 32	1 30	1 22	1 16	1 14	1 3	0 54	1 22·6
8 30	1 56·4	1 54	1 52·7	1 51·2	1 46·5	1 44·2	1 34·4	1 36	1 22·9	1 18	1 41·3
9 0	2 4·2	2 6	2 7	2 6	2 0	1 55	1 50	1 50	1 42	1 36	1 56·6
9 30	2 8	2 13·6	2 16·2	2 13·4	2 10·5	2 2·7	2 1	2 3	1 55	1 48	2 5·1
10 0	2 14·5	2 18	2 16	2 18	2 15	2 8	2 6	2 6	2 1·5	1 54	2 9·7
10 30	2 19·8	2 18·3	2 15	2 19·2	2 14·6	2 11·3	2 5·9	2 5·6	2 2·4	1 56·4	2 10·8
11 0	2 16	2 16	2 13	2 15	2 11	2 8	2 4	2 2·5	1 58·5	1 55	2 7·9
11 30	2 11	2 10·4	2 9·3	2 8·5	2 5	2 2·6	1 59·7	1 56·8	1 59·6	1 51·2	2 2·7

Table XXXIX. (Mean of Tables IX. and XXX.)

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north or south; from 10,796 observations.

Moon's Transit. A.M.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·42	22·44	22·58	22·49	22·61	22·36	22·32	22·28	22·13	22·09	22·37
0 30	22·68	22·47	22·80	22·64	22·64	22·71	22·57	22·55	22·29	22·39	22·57
1 0	22·85	22·62	22·94	22·76	22·77	22·86	22·75	22·77	22·49	22·39	22·66
1 30	22·99	22·84	23·09	22·85	22·92	22·92	22·93	22·99	22·68	22·38	22·85
2 0	22·90	22·84	23·00	22·87	22·95	22·78	23·20	22·83	22·72	22·52	22·86
2 30	22·81	22·83	22·85	22·91	22·93	22·63	23·44	22·67	22·64	22·64	22·83
3 0	22·70	22·72	22·68	22·67	22·78	22·58	22·91	22·50	22·37	22·45	22·64
3 30	22·58	22·56	22·49	22·37	22·56	22·54	22·38	22·32	22·10	22·27	22·42
4 0	22·12	22·26	22·18	22·10	22·10	22·17	22·08	22·08	21·80	22·04	22·09
4 30	21·67	21·95	21·85	21·82	21·63	21·81	21·77	21·84	21·50	21·81	21·76
5 0	21·32	21·48	21·50	21·36	21·31	21·39	21·35	21·27	21·14	21·20	21·33
5 30	21·00	20·95	21·14	20·81	21·02	20·96	20·92	20·69	20·78	20·60	20·89
6 0	20·37	20·56	20·76	20·37	20·59	20·42	20·42	20·30	20·14	20·05	20·40
6 30	19·77	20·16	20·34	20·11	20·12	19·86	19·93	19·89	19·52	19·50	19·92
7 0	19·82	19·99	19·88	19·97	19·69	19·61	19·62	19·39	19·32	19·17	19·65
7 30	19·87	19·91	19·58	19·84	19·42	19·45	19·32	18·89	19·13	18·85	19·43
8 0	19·91	20·12	19·83	19·68	19·75	19·64	19·47	19·18	19·14	18·87	19·56
8 30	20·00	20·33	20·08	19·53	20·08	19·83	19·64	19·52	19·29	19·07	19·74
9 0	20·45	20·66	20·47	20·17	20·43	20·19	20·04	20·0	19·69	19·50	20·16
9 30	20·93	20·99	20·87	20·83	20·78	20·60	20·43	20·49	20·11	19·93	20·60
10 0	21·36	21·45	21·36	21·36	21·25	21·05	20·88	20·84	20·61	20·42	21·06
10 30	21·79	21·89	21·84	21·88	21·70	21·50	21·33	21·19	21·12	20·91	21·51
11 0	21·99	22·16	22·10	22·12	22·13	21·78	21·69	21·60	21·52	21·33	21·94
11 30	22·16	22·99	22·37	22·32	22·56	22·03	22·06	22·00	21·90	21·76	22·21

Table XL.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every three degrees of her Declination.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 0	+ 7	+ 7	+ 6	+ 5	+ 2	− 1	− 3	− 6	− 9	−11
0 30	+ 6	+ 7	+ 6	+ 5	+ 2	− 1	− 2	− 4	− 8	−10
1 0	+ 6	+ 5	+ 6	+ 5	+ 3	+ 1	− 3	− 6	− 7	−10
1 30	+ 5	+ 1	+ 5	+ 4	+ 4	+ 1	− 6	− 8	− 7	−11
2 0	+ 6	+ 3	+ 6	+ 6	+ 4	+ 3	− 3	− 6	− 5	−10
2 30	+ 6	+ 5	+ 6	+ 8	+ 3	+ 2	− 1	− 5	− 6	−10
3 0	+ 7	+ 6	+ 7	+ 7	+ 3	+ 1	− 2	− 6	− 6	−12
3 30	+ 9	+ 9	+ 8	+ 7	+ 4	0	− 2	− 5	− 7	−13
4 0	+11	+10	+ 9	+ 8	+ 6	0	− 2	− 5	− 7	−15
4 30	+11	+ 9	+ 9	+ 9	+ 7	0	+ 2	− 5	−10	−17
5 0	+15	+12	+12	+12	+ 9	+ 1	− 1	− 4	−10	−16
5 30	+16	+13	+12	+13	+ 7	0	− 3	− 7	−11	−18
6 0	+18	+16	+13	+14	+ 8	+ 2	− 3	− 7	−14	−20
6 30	+19	+19	+18	+13	+ 9	+ 3	− 4	− 9	−18	−21
7 0	+19	+19	+15	+13	+ 9	+ 2	− 4	− 8	−18	−26
7 30	+18	+17	+15	+11	+ 9	− 1	− 4	− 8	−18	−30
8 0	+17	+16	+13	+ 9	+ 7	− 1	− 7	− 9	−20	−29
8 30	+17	+15	+14	+12	+ 7	+ 5	− 5	− 3	−16	−21
9 0	+ 8	+10	+11	+10	+ 4	− 1	− 6	− 6	−14	−20
9 30	+ 3	+ 9	+11	+ 8	+ 5	− 2	− 4	− 2	−10	−17
10 0	+ 5	+ 8	+ 6	+ 8	+ 5	− 2	− 4	− 4	− 8	−16
10 30	+10	+ 8	+ 5	+ 9	+ 5	+ 1	− 4	− 4	− 8	−14
11 0	+ 8	+ 8	+ 5	+ 7	+ 3	0	− 4	− 5	− 9	−13
11 30	+ 8	+ 7	+ 6	+ 5	+ 2	0	− 3	− 6	−10	−12

Table XLI.

Showing the Difference in the Height of High Water and the Mean Height (Column B. Table XXIV.) for every three degrees of the Moon's Declination.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	+·10	+ ·12	+·26	+·17	+·29	+·04	.....	−·04	−·19	−·23
0 30	+·10	− ·11	+·22	+·08	+·06	+·13	−·01	−·03	−·29	−·19
1 0	+·14	− ·09	+·23	+·05	+·06	+·15	+·04	+·06	−·22	−·32
1 30	+·18	− ·01	+·28	+·04	+·11	+·11	+·12	+·18	−·13	−·43
2 0	+·08	+ ·02	+·18	+·05	+·13	−·04	+·38	+·01	−·10	−·30
2 30	+·09	+ ·11	+·13	+·19	+·21	−·09	+·72	−·05	−·08	−·08
3 0	+·10	+ ·12	+·08	+·07	+·18	−·02	+·31	−·10	−·23	−·15
3 30	+·20	+ ·18	+·11	−·01	+·18	+·16	.....	−·06	−·28	−·11
4 0	+·02	+ ·16	+·08	.....	.....	+·07	−·02	−·02	−·30	−·06
4 30	−·04	+ ·24	+·14	+·11	−·08	+·10	+·06	+·13	−·21	+·10
5 0	.....	+ ·16	+·18	+·04	−·01	+·07	+·03	−·05	−·18	−·12
5 30	+·14	+ ·09	+·28	−·05	+·16	+·10	+·06	−·17	−·08	−·26
6 0	+·02	+ ·21	+·41	+·02	+·24	+·07	+·07	−·05	−·21	−·30
6 30	−·15	+ ·24	+·42	+·19	+·20	−·06	+·01	−·03	−·40	−·42
7 0	+·26	+ ·43	+·32	+·41	+·13	+·05	+·06	−·17	−·24	−·39
7 30	+·50	+ ·54	+·21	+·47	+·05	+·08	−·05	−·48	−·24	−·52
8 0	+·47	+ ·68	+·39	+·24	+·31	+·20	+·03	−·26	−·30	−·57
8 30	+·33	+ ·66	+·41	−·14	+·41	+·16	−·03	−·15	−·38	−·60
9 0	+·25	+ ·46	+·27	−·03	+·23	−·01	−·16	−·20	−·51	−·70
9 30	+·41	+ ·47	+·35	+·31	+·26	+·08	−·09	−·03	−·41	−·59
10 0	+·42	+ ·51	+·42	+·42	+·31	+·11	−·06	−·10	−·33	−·52
10 30	+·37	+ ·47	+·42	+·46	+·28	+·08	−·09	−·23	−·30	−·51
11 0	+·25	+ ·42	+·36	+·38	+·39	+·04	−·05	−·14	−·22	−·41
11 30	+·18	+1·01	+·39	+·34	+·58	+·05	+·08	+·02	−·08	−·22

Table XLII.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every minute of her Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	+12	+10	+ 3	+ 2	− 2	− 6	− 9	−13
1 30	+ 8	+ 6	+ 3	+ 1	− 2	− 4	− 7	−12
2 30	+ 7	+ 5	+ 3	0	+ 2	− 5	− 5	−18
3 30	+ 6	+ 3	+ 3	+ 2	0	− 4	− 6	
4 30	+ 1	+ 2	+ 3	+ 1	− 1	− 3	− 8	
5 30	0	+ 1	0	+ 1	− 2	− 2		
6 30	+ 5	+ 3	0	0	− 3	− 3		
7 30	+ 9	+ 3	+ 3	− 3	− 5	− 8		
8 30	+19	+10	+ 5	− 2	− 4	− 7	−12	
9 30	+17	+12	+ 7	− 1	− 5	−12	−17	
10 30	+15	+13	+ 7	− 1	− 2	− 9	−14	−16
11 30	+13	+ 9	+ 6	0	− 3	− 5	−14	−16

Table XLIII.

Showing the Difference in the Height of High Water and the Mean Height (Column B. Table XXIV.) for every minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	−·45	−·35	−·35	+·07	+·15	+·07	+·33	+·59
1 30	−·55	−·49	−·03	−·05	+·26	+·31	+·45	+·60
2 30	−·66	−·48	−·32	+·01	+·20	+·41	+·62	+·82
3 30	−·71	−·41	−·25	+·04	+·12	+·53	+·85	
4 30	−·74	−·57	−·22	−·02	+·39	+·68	+·80	
5 30	−·72	−·53	−·12	+·22	+·10	+·74		
6 30	−·73	−·33	−·20	+·02	+·32	+·56		
7 30	−·63	−·35	−·25	+·09	+·28	+·49	+·67	
8 30	−·34	−·20	−·07	−·03	+·08	+·38	+·50	
9 30	−·38	−·29	−·12	+·19	+·17	+·22	+·55	+·59
10 30	−·30	−·40	−·20	−·09	+·11	+·27	+·40	+·40
11 30	−·37	−·17	−·14	+·14	+·28	+·54	+·47	+·59

The following Tables may be used in predicting the phenomena of the tides, with the argument of the moon's transit in apparent time.

For the Time of High Water at the London Docks.

Table III., column A., containing the semimenstrual inequality + a constant, which is again inserted here for the convenience of reference.

Table XLIV. containing the correction for the moon's parallax.

Table XLV. containing the correction for the moon's declination.

The last two Tables have been obtained from Tables XL. and XLII., by removing arbitrarily the irregularities which those Tables present.

Table XLVI., formed by arbitrary alterations from Table G., p. 148, gives the correction for the calendar months.

To the result obtained must be added the equation of time, in order to have the time of high water sought in mean time.

Tables XLIII. and XLIV. may probably be safely employed for all the ports in the United Kingdom. It should be borne in mind that these Tables cannot in any case be depended upon to within two or three minutes, from the great irregularities of the phenomena to which they refer, and from the difficulty of ascertaining by observation the precise time of high water. The observations upon which they are founded are only recorded to the nearest five minutes, and they were not always made with so much care as might have been desired.

For the Height of High Water at the London Docks.

Table XXIV., column B., containing the semimenstrual inequality + a constant.

Table XLVII. containing the correction for the moon's parallax.

Table XLVIII. containing the correction for the moon's declination.

The last two Tables have been formed by arbitrary changes from Tables XLIII. and XLII., which Tables present great irregularities; for the height of high water is subject to much greater irregularity than the time. The effect of changes in the moon's parallax upon the height appears to be considerably greater than that of changes in her declination.

COLUMN A. TABLE III.

Containing the semimenstrual inequality + a constant, and showing the Interval between the Moon's Transit and the Time of High Water, from the Philosophical Transactions, 1831, p. 401.

Moon's Transit.	h 0	h 1	h 2	h 3	h 4	h 5	h 6	h 7	h 8	h 9	h 10	h 11
Interval	h m 1 57	h m 1 42	h m 1 26	h m 1 11	h m 0 56	h m 0 45	h m 0 42	h m 0 52	h m 1 23	h m 1 56	h m 2 10	h m 2 8

TABLE XLIV.

Showing the Correction for the Moon's Parallax, formed by arbitrary alterations from Table XLII.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	Moon's Transit.
h m	m	m	m	m	m	m	m	h m
0 0	+11	+ 7	+ 3	0	− 3	− 7	−11	0 0
0 30	+11	+ 7	+ 3	0	− 3	− 7	−11	0 30
1 0	+10	+ 6	+ 3	0	− 3	− 6	−10	1 0
1 30	+ 9	+ 6	+ 3	0	− 3	− 6	− 9	1 30
2 0	+ 8	+ 5	+ 2	0	− 2	− 5	− 8	2 0
2 30	+ 7	+ 4	+ 2	0	− 2	− 4	− 7	2 30
3 0	+ 6	+ 4	+ 2	0	− 2	− 4	− 6	3 0
3 30	+ 5	+ 4	+ 2	0	− 2	− 4	− 5	3 30
4 0	+ 4	+ 3	+ 1	0	− 1	− 3	− 4	4 0
4 30	+ 3	+ 2	+ 1	0	− 1	− 2	− 3	4 30
5 0	+ 2	+ 1	0	0	0	− 1	− 2	5 0
5 30	+ 1	0	0	0	0	0	− 1	5 30
6 0	+ 1	0	0	0	0	0	− 1	6 0
6 30	+ 4	+ 3	+ 1	0	− 1	− 3	− 4	6 30
7 0	+ 6	+ 4	+ 2	0	− 2	− 4	− 6	7 0
7 30	+ 9	+ 5	+ 2	0	− 2	− 5	− 9	7 30
8 0	+12	+ 8	+ 4	0	− 4	− 8	−12	8 0
8 30	+15	+10	+ 5	0	− 5	−10	−15	8 30
9 0	+17	+11	+ 6	0	− 6	−11	−17	9 0
9 30	+17	+11	+ 6	0	− 6	−11	−17	9 30
10 0	+15	+10	+ 5	0	− 5	−10	−15	10 0
10 30	+14	+10	+ 5	0	− 5	−10	−14	10 30
11 0	+13	+ 9	+ 4	0	− 4	− 9	−13	11 0
11 30	+12	+ 8	+ 4	0	− 4	− 8	−12	11 30

Table XLV.

Showing the Correction for the Moon's Declination, formed from Table XL.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	h m
0 0	+ 8	+ 6	+ 4	+ 2	+ 1	0	− 3	− 6	− 9	−12	0 0
0 30	+ 7	+ 6	+ 4	+ 2	+ 1	0	− 2	− 6	− 9	−12	0 30
1 0	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 2	− 6	− 9	−11	1 0
1 30	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9	−11	1 30
2 0	+ 5	+ 4	+ 3	+ 2	+ 1	0	− 3	− 6	− 9	−10	2 0
2 30	+ 6	+ 5	+ 4	+ 3	+ 2	0	− 2	− 5	− 6	−11	2 30
3 0	+ 7	+ 5	+ 4	+ 3	+ 2	0	− 2	− 5	− 6	−12	3 0
3 30	+ 8	+ 6	+ 4	+ 2	+ 1	0	− 3	− 6	− 8	−13	3 30
4 0	+ 9	+ 7	+ 5	+ 5	+ 2	0	− 3	− 7	−11	−15	4 0
4 30	+12	+10	+ 8	+ 6	+ 3	0	− 3	− 7	−11	−16	4 30
5 0	+15	+12	+ 9	+ 6	+ 3	0	− 3	− 7	−11	−17	5 0
5 30	+17	+15	+11	+ 7	+ 3	0	− 3	− 7	−13	−18	5 30
6 0	+18	+16	+12	+ 8	+ 3	0	− 4	− 9	−15	−20	6 0
6 30	+19	+17	+12	+ 8	+ 3	0	− 5	−10	−16	−23	6 30
7 0	+19	+17	+12	+ 8	+ 3	0	− 5	−12	−21	−26	7 0
7 30	+18	+16	+11	+ 8	+ 3	0	− 6	−12	−18	−28	7 30
8 0	+17	+15	+10	+ 7	+ 2	0	− 5	−12	−18	−28	8 0
8 30	+16	+15	+10	+ 7	+ 2	0	− 4	−10	−15	−23	8 30
9 0	+15	+12	+ 9	+ 6	+ 3	0	− 3	− 8	−14	−21	9 0
9 30	+13	+11	+ 8	+ 4	+ 1	0	− 4	− 8	−13	−18	9 30
10 0	+12	+10	+ 7	+ 5	+ 1	0	− 4	− 8	−12	−17	10 0
10 30	+11	+ 9	+ 6	+ 3	+ 1	0	− 3	− 6	−10	−15	10 30
11 0	+10	+ 8	+ 5	+ 2	+ 1	0	− 3	− 6	− 9	−14	11 0
11 30	+ 9	+ 7	+ 4	+ 2	+ 1	0	− 3	− 8	− 8	−13	11 30

Table XLVI.

Showing the correction for the calendar months, formed from Table G, p. 148.

Moon's Transit.	Jan. July.	Feb. Aug.	March. Sept.	April. Oct.	May. Nov.	June. Dec.	Moon's Transit.
h	m	m	m	m	m	m	h
0	− 2	− 2	0	+ 2	+ 2	0	0
1	− 3	− 2	0	+ 3	+ 2	0	1
2	− 7	− 2	+ 2	+ 7	+ 2	− 2	2
3	− 4	0	+ 3	+ 4	0	− 3	3
4	0	+ 1	+ 4	0	− 1	− 4	4
5	+ 3	+ 5	+ 6	− 3	− 5	− 6	5
6	+ 8	+ 6	0	− 8	− 6	0	6
7	+11	+ 5	− 3	−11	− 5	+ 3	7
8	+ 8	+ 2	− 5	− 8	− 2	+ 5	8
9	+ 3	− 1	− 1	− 3	+ 1	+ 1	9
10	0	− 1	0	0	+ 1	0	10
11	− 1	− 1	− 1	+ 1	+ 1	+ 1	11

Table XLVII.

Showing the correction for the Moon's Parallax in the Height of High Water, formed by arbitrary alterations from Table XLIII.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	Moon's Transit.
h m	Feet.	Feet.	Feet.		Feet.	Feet.	Feet.	h m
0 0	−·42	−·28	−·14	0	+·14	+·28	+·42	0 0
1 0	−·45	−·30	−·15	0	+·15	+·30	+·45	1 0
2 0	−·55	−·36	−·18	0	+·18	+·36	+·55	2 0
3 0	−·65	−·44	−·22	0	+·22	+·44	+·64	3 0
4 0	−·72	−·48	−·24	0	+·24	+·48	+·72	4 0
5 0	−·75	−·50	−·25	0	+·25	+·50	+·75	5 0
6 0	−·72	−·48	−·24	0	+·24	+·48	+·72	6 0
7 0	−·67	−·44	−·22	0	+·22	+·44	+·67	7 0
8 0	−·62	−·40	−·20	0	+·20	+·40	+·62	8 0
9 0	−·57	−·38	−·19	0	+·19	+·38	+·57	9 0
10 0	−·52	−·36	−·18	0	+·18	+·36	+·52	10 0
11 0	−·47	−·32	−·16	0	+·16	+·32	+·47	11 0

Table XLVIII.

Showing the Correction for the Moon's Declination in the Height of High Water, formed by arbitrary alterations from Table XLI.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	Feet.	Feet.	Feet.	Feet.	Feet.		Feet.	Feet.	Feet.	Feet.	h m
0 0	+·25	+·20	+·15	+·10	+·05	0	−·05	−·10	−·15	−·20	0 0
1 0	+·17	+·13	+·10	+·07	+·03	0	−·03	−·06	−·09	−·30	1 0
2 0	+·12	+·09	+·06	+·04	+·02	0	−·02	−·04	−·06	−·09	2 0
3 0	+·07	+·05	+·03	+·02	+·01	0	−·01	−·02	−·03	−·05	3 0
4 0	+·02	+·02	+·01	0	0	0	0	0	−·01	−·02	4 0
5 0	+·05	+·04	+·03	+·02	+·01	0	−·01	−·02	−·03	−·04	5 0
6 0	+·15	+·12	+·09	+·06	+·03	0	−·03	−·06	−·09	−·12	6 0
7 0	+·25	+·29	+·15	+·10	+·05	0	−·05	−·10	−·15	−·20	7 0
8 0	+·50	+·40	+·30	+·20	+·10	0	−·10	−·20	−·30	−·40	8 0
9 0	+·50	+·40	+·30	+·20	+·10	0	−·10	−·20	−·30	−·40	9 0
10 0	+·42	+·33	+·24	+·16	+·08	0	−·08	−·16	−·24	−·33	10 0
11 0	+·35	+·28	+·21	+·14	+·07	0	−·07	−·14	−·21	−·28	11 0

The last Tables, intended to serve for the prediction of the tides in the port of London, seem to me fairly to embody the results furnished by more than 10,000 observations. They might perhaps be amended in a subsequent revision, by applying these corrections, considered as approximate, to each of the observations employed, with a contrary sign, so as to obtain a discussion of their errors. I am confident, however, that they are not susceptible of any material improvement; and that we may proceed safely to investigate the laws or expressions which they represent. It will also be desirable to ascertain, by a discussion of their errors, when used in predicting the phenomena, whether the fluctuations of the atmosphere, as indicated by the barometer, have any sensible effect, as is the case, according to M. Daussy, on the coast of France, and also to discover the extent to which the phenomena are modified by winds and storms.

The Tables might be rendered more convenient for practice by the addition of a constant, so as to render all the corrections positive, but I have retained them in their present state in order that the law of the inequalities may be more apparent. This change can easily be made if required, but by such a process the intrinsic character of the Tables is not altered.

In the Philosophical Transactions for 1833 I gave the semimenstrual inequality, including of course the establishment for Brest, Plymouth, Portsmouth, and Sheerness. M. Dessiou has since deduced the following Tables for Pembroke Dockyard, Liverpool, Howth, and Ramsgate.

Pembroke Dockyard.			Liverpool Docks.			Howth Harbour.			Ramsgate Harbour.		
Moon's Transit.	Correspond. ing Interval.	No. of Obs.	Moon's Transit.	Correspond. ing Interval.	No. of Obs.	Moon's Transit.	Correspond. ing Interval.	No. of Obs.	Moon's Transit.	Correspond. ing Interval.	No. of Obs.
h m	h m		h m	h m		h m	h m		h m	h m	
0 14·8	6 0	29	0 17·2	11 17·4	56	0 18	10 57	8	0 13	11 41·5	32
0 45·1	5 53·7	30	0 45·5	11 11·1	60	0 44	10 55	7	0 43	11 40·4	29
1 16·1	5 46·9	30	1 15·5	11 4·3	55	1 12	10 47	8	1 15·2	11 32·3	33
1 46·5	5 39·5	29	1 44·7	10 57	59	1 45	10 30	9	1 42·2	11 31·8	26
2 15·7	5 30·3	27	2 15·3	10 58·5	59	2 19·5	10 22·5	8	2 15·6	11 12·4	30
2 44	5 23·5	27	2 45·1	10 43·8	59	2 48	10 10	7	2 45·1	11 9·4	27
3 14·9	5 15·6	30	3 14·4	10 38·2	57	3 13	10 14	5	3 15·7	11 2·7	32
3 44·5	5 14·3	26	3 44·6	10 30·4	63	3 45	10 10	9	3 45·4	10 54·2	26
4 12·7	5 8·6	28	4 15·5	10 26·7	61	4 19	10 6	7	4 15·5	10 43·1	32
4 44·5	5 1·4	34	4 45·9	10 24·8	62	4 45	10 4	8	4 45·3	10 41·6	28
5 17	4 59·7	28	5 16	10 25·5	58	5 16	10 22	8	5 17·3	10 38·7	34
5 45·5	4 55·3	28	5 45·2	10 26·5	55	5 45	10 16	9	5 46·7	10 34·6	26
6 14	5 1·7	28	6 13·8	10 34·2	54	6 13·3	10 26	14	6 17	10 44·9	32
6 42·7	5 9·1	27	6 44·5	10 49·5	65	6 47	10 33	18	6 43·9	10 56·6	31
7 13·7	5 26·7	33	7 16·6	11 6·5	57	7 19	10 58	13	7 14	11 1·9	48
7 46·3	5 47·7	33	7 45·5	11 21	54	7 47	11 3	13	7 47	11 11·1	32
8 16·4	6 2·6	27	8 14·7	11 36·5	58	8 15	11 17	14	8 18	11 31·6	29
8 44·5	6 10	29	8 44	11 45·6	58	8 44	11 27	13	8 45·5	11 53·8	27
9 16·2	6 20·5	31	9 16·4	11 46·4	59	9 13·3	11 32	15	9 14·3	11 53·9	31
9 47·5	6 23·1	31	9 43·7	11 48·5	58	9 43	11 33	13	9 47	12 2·8	33
10 16·7	6 23·3	26	10 14·8	11 44·6	59	10 13·5	11 33·5	15	10 18	11 54·3	27
10 45	6 20·6	29	10 43·5	11 39·4	55	10 45·7	11 29	13	10 44·5	11 59·2	26
11 13	6 15·6	27	11 11·5	11 34·6	58	11 17·0	11 23	10	11 15·1	11 52·6	31
11 42·6	6 8·7	30	11 44·3	11 26·4	63	11 45	11 17	10	11 44·3	11 52·6	28

The Pembroke tides are from 697 observations made between the 1st of November 1832 and the 31st of October 1833. Those for the Liverpool Docks are from 1402 observations made in the years 1772 and 1791. Those for Howth Harbour are from only 254 observations, made in 1817 between the 16th of June and the 15th of December. Those for Ramsgate Harbour are from 730 observations, made between the 1st of September 1831 and the 31st of August 1833.

TABLE showing the Interval between the Moon's Transit and the Time of High Water.

Moon's Transit.	Pembroke.	Liverpool.	Howth.	Ramsgate.	Moon's Transit.
h m	h m	h m	h m	h m	h m
0 0	6 4	11 22	11 8	11 46	0 0
0 30	5 57	11 14	11 0	11 41	0 30
1 0	5 50	11 8	10 50	11 36	1 0
1 30	5 43	11 1	10 40	11 30	1 30
2 0	5 35	10 54	10 28	11 19	2 0
2 30	5 27	10 47	10 18	11 11	2 30
3 0	5 20	10 41	10 13	11 5	3 0
3 30	5 15	10 34	10 10	10 59	3 30
4 0	5 11	10 29	10 8	10 49	4 0
4 30	5 5	10 26	10 7	10 42	4 30
5 0	5 1	10 25	10 8	10 39	5 0
5 30	4 57	10 26	10 12	10 36	5 30
6 0	4 58	10 30	10 20	10 40	6 0
6 30	5 5	10 42	10 29	10 50	6 30
7 0	5 18	10 58	10 42	10 59	7 0
7 30	5 37	11 13	10 56	11 7	7 30
8 0	5 55	11 29	11 10	11 20	8 0
8 30	6 7	11 41	11 22	11 41	8 30
9 0	6 16	11 46	11 30	11 54	9 0
9 30	6 22	11 48	11 33	11 58	9 30
10 0	6 23	11 46	11 33	11 59	10 0
10 30	6 22	11 42	11 31	11 57	10 30
11 0	6 18	11 37	11 26	11 56	11 0
11 30	6 12	11 30	11 19	11 53	11 30

X. *On the Nature of Death.* *By* A. P. W. Philip, *M.D. F.R.S. L. & E. &c.*

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I NEED hardly say, that in such a communication as the present, I have no intention of entering into the part of the subject of this paper which may justly be termed metaphysical. The veil which separates it from experimental science must ever remain impenetrable, there being no source of information respecting it, but a direct revelation from the great Author of our being, or the instincts he has implanted in our nature, for all knowledge is not acquired. We come into the world with knowledge essential to our existence. The infant knows as well how to breathe and how to suck as the adult, and these acts depend as much on mental operations as those which are the results of experience. He perceives his wants, and he knows how to relieve them; and the extent to which this species of knowledge exists in some animals, whose reasoning powers are extremely limited, justly excites our wonder and admiration. They know what is essential to their condition with an accuracy which sets at defiance all the efforts of human reasoning, for their knowledge is the knowledge of their Creator.

To the physiological part of the subject alone I wish to direct the attention of the Society. It forms part of the same subject with the three last papers I had the honour to present to it, published in the Philosophical Transactions for 1831 and 1833; namely, the relation which the different powers of the living animal body bear to each other. In these papers I endeavoured to trace the nature of their influence on each other while their state of vigour remains; in the following paper I shall attempt to point out the manner in which they influence each other in their state of decay.

In the course of my Inquiry into the Laws of the Vital Functions, it became necessary to determine, with more precision than had been done, the line of distinction between the sensorial and nervous functions.

The function of the muscular system, from its nature and the peculiar structure of its organs, is readily defined; but in the nervous system we perceive more than one set of functions, and yet, both from the variety of ways in which they are interwoven, and from the peculiar mechanism of the active parts of their organs being so minute as to escape our senses and consequently the investigations of the anatomist, the difficulty of correctly distinguishing them is considerable. It is only by experiments instituted for the purpose, and founded on the very different nature of these sets of functions, that the line of distinction can be drawn.

In order to render the results more certain, I endeavoured to ascertain this line by two sets of experiments, conducted on different principles; the object of the one being

to ascertain what functions remain after the sensorial power is withdrawn, and of the other, what functions fail on withdrawing the nervous power; and in prosecuting this subject, I found it requisite to study the process of dying, to determine the steps by which the body of the more perfect animal becomes subject to the laws of inanimate matter.

The experiments by which this was more immediately attempted were not laid before the Society as were the other parts of the investigation. They are detailed at length in the second part of my Inquiry into the Laws of the Vital Functions. I have there, however, entered no further into the nature of death than was necessary for the purpose I then had in view. I am now about to compare the results of these experiments with those of others, made since the publication of that treatise, with a view, as far as experiment can apply to it, of explaining the nature of death.

It appears to me that the various facts ascertained in the course of the inquiries in which I have been so long engaged, throw light on this subject. I shall, as I proceed, refer to the passages, either in my papers in the Philosophical Transactions or my Inquiry into the Laws of the Vital Functions, where the proofs of the different positions I shall have occasion to state, will be found.

In the last of the papers above referred to, I had occasion to observe, that there is no question relating to the animal economy which involves a more general view of its phenomena than the nature of sleep. The nature of death also includes a general view of the functions of health, for such we shall find are the laws of our frame, that these functions alone necessarily lead to death; but the nature of death is a more complicated question. It includes the various ways in which the functions are influenced by disease, the effects of which are so numerous that they seem at first view a train of countless phenomena which defy all attempts to refer them to general principles.

I need not say that many advantages would arise from a correct knowledge of the immediate cause of death, and of the different sources from which the state that constitutes that cause arises. The most important would be, that it would give to the physician a clearer view of the tendencies of disease, and consequently of the indications of cure; but it would not be the least of its advantages, that it would tend to strip a change which all must undergo of the groundless terrors with which, we have reason to believe, the timid and fanciful have clothed it.

IT appears from the experiments in question, that in the more perfect animals there are three distinct classes of functions, the sensorial, the nervous, and the muscular, which, having no direct dependence, are yet, through their organs, dependent on each other; for the destruction of any one of these classes of functions more or less immediately destroys the organs of all.

We know that the immediate organs of the nervous and sensorial functions, although both residing in the brain and spinal marrow, are distinct sets of organs, because they

have not the same locality; the former, as appears from direct experiments, being distributed throughout the whole brain and spinal marrow, and, as far as experiment can determine, equally so, except that the lower part of the spinal marrow either partakes of them less, or they are there of less power*; while the latter, in all the more perfect animals, are chiefly, and in man almost wholly, confined to the brain; and because in disease we often see the functions of the one class greatly impaired without those of the other being at all affected, and in the process of dying, we shall find all the sensorial functions finally lost, while all the nervous functions remain, and are only indirectly impaired by the loss of the former.

The sensorial functions constitute the sensitive system,—that by which we perceive and act,—and consequently are connected with the world which surrounds us. The nervous and muscular, the vital system, that by which we are maintained.

From the same experiments it appears, that what is called death consists in the loss of the first of these classes of functions, the sensorial, the nervous and muscular functions still continuing, which are lost only in consequence of the failure of respiration, the only vital function to which the cooperation of the sensorial power is necessary.

Many hypotheses have been framed for the purpose of explaining why the motions of the heart and blood-vessels are not, like those of the limbs, subjected to the will. Among these is the hypothesis of Dr. Johnstone of Worcester†, adopted from Winslow, Prochaska, and other writers, which professes to rest on the evidence of experiment, and ascribes to the ganglions the power of intercepting the influence of the brain, and consequently of the will. We have seen, however, that the influence of both the brain and spinal marrow reaches the heart and blood-vessels as readily as the muscles of voluntary motion‡.

All that has been written on this question seems only to perplex it. When we dismiss the various hypotheses on the subject, the answer appears easy. There are evidently two conditions necessary to render a muscle subject to the will: the stimulus which excites it must be so, and it must be capable of effecting an end desired. If we had no wish to handle, the muscles of the hand would never have become subject to the will. The heart and blood-vessels in all their usual motions are excited by the blood, the stimulating properties of which the will can neither increase nor impair; and what act of volition could be performed by these organs? The only internal organs which can effect an end desired, are the rectum and bladder, when their contents have accumulated to a certain extent; and they are both, under such circumstances, subjected to the will. Their action here may be said to be vital functions, to which the cooperation of the sensorial power is necessary; but, to say nothing of the sensorial

* Philosophical Transactions for 1815, 1829, and 1833; and my Experimental Inquiry into the Laws of the Vital Functions, Part II. chap. ii. Third Edition: wherever this Inquiry is referred to, the references are to the Third Edition.

† Essay on the Use of the Ganglions; published in 1771.

‡ Philosophical Transactions for 1815; and Experimental Inquiry, Part II. chap. i. and ii.

power under all circumstances not being essential to them, they are not so immediately essential to life as to be comprehended in the term vital functions, according to its usual acceptation.

When the animal no longer feels and wills, his breathing ceases and he is, according to the common acceptation of the term, dead, although his body still retains its other powers, which, while they last, prevent its obeying the laws of inanimate nature*; but the changes which after this take place, of course no more affect the individual than if they took place in any other mass of matter.

In inquiring into the physiological nature of what is called death, therefore, it is to the ceasing of the sensorial functions alone that the attention must be directed. Thus the subject divides itself into two parts; the final loss of the sensorial functions, which in common language has obtained the name of Death; and absolute death, that is, the loss of all the functions, which we shall find in the more perfect animals is the necessary consequence of the loss of the sensorial functions.

The latter functions, as I have already had occasion to point out in my paper on the Nature of Sleep, published in the first part of the Philosophical Transactions for 1833, belong to those parts of the brain and spinal marrow which are associated with the nerves of the sensitive system, and which, it appears, from another paper which the Society did me the honour to publish in the same part of the Transactions, are the only active parts of the sensorial system; those on which the power of all its other parts depends. To them, therefore, we must look for the immediate cause of failure when the functions of the sensitive system, whether temporarily or finally, fail. It is here we found that the immediate cause of sleep exists; and it appears, from what has just been said, that to the same parts we must look for the immediate cause of what is called death.

The state which immediately precedes the last act of dying, then, according to the common acceptation of the term, and sleep, depend on a failure of function in the same organs. In what, then, consists the difference of these states? The most evident is, that the one is a temporary, the other a final failure; and it will appear, that in the only death which can strictly be called natural, the state of the sensitive system which immediately precedes death differs from its state in sleep in no respect but in degree.

The cause of sleep, as appears from the paper above referred to, is uniformly the same,—a diminished excitability of the sensitive parts of the brain and spinal marrow, in consequence of the action of the ordinary stimulants of life; but a loss of excitability in those parts we shall find is never the sole cause of death, and often makes no part of its cause. In sleep we have seen that the sensitive parts of the brain and spinal marrow regain their functions in consequence of the continued vigour of the vital system, by which their excitability is restored. To render the exhaustion which constitutes sleep permanent, therefore, the powers of this system also must fail;

* Experimental Inquiry, Part II. chap. xi.

and if any cause of failure in these powers occur, it is evident, that whatever be the state of the sensitive system, its powers must fail with them.

THE natural death of the animal is the death of old age; and as this is the simplest form of death, it is that which I shall first consider. We shall find that the state which immediately precedes this death, and must consequently be considered as its cause, must, in the nature of things, differ from sleep in no other respect than the less vigorous state of the functions of both systems, and consequently that these states are identical; the greater or less general vigour making no difference in their nature.

We are not necessarily born to suffering. All natural states, with the exception of child-bearing, (and in its most natural state even this is hardly an exception,) are more or less pleasurable. It will appear from the nature of our constitutions, that the last feelings in natural death are necessarily of the same nature as those which precede sleep. It is only where the course of our decay is disturbed, that suffering of any kind attends it.

From a knowledge of the animal economy, we might, independently of experience, have foretold that a state of sleep would be that which immediately precedes the last act of dying from old age. It appears from what was said of the nature of sleep in the paper above referred to, that although the vital organs do not, in it, partake of the peculiar state which constitutes sleep, their functions are all, for the time, impaired by the exhaustion of the sensitive system. The respiration, we have seen, is rendered less frequent, in consequence of which the activity both of the circulation and the other assimilating functions which depend on it, is, for the time, lessened.

Now, as the death of old age arises from the gradual failure of those functions, it must necessarily take place at the time at which their vigour is most impaired. If the vital powers are still capable of restoring the sensitive system under the disadvantage of a diminished frequency of respiration, it is evident that, if their decay be gradual, nothing occurring suddenly to accelerate it, they cannot fail to maintain the functions of that system during the short time which intervenes before the recurrence of sleep again exposes them to the same difficulty. Their failure necessarily takes place at the time when their functions are most difficult. The death of old age, therefore, is literally the last sleep, uncharacterized by any peculiarity. The general languor of the functions in the last waking interval is attended with no peculiar suffering, and the last sleep commences with the usual grateful feelings of repose, the last feelings experienced; for with what takes place after them, the feelings, being suspended, have no concern.

The only difference between the last, and the sleep of former times, is, that the exhaustion of the sensitive system, which is at first, as in the latter case, only partial, (for in the beginning of this sleep the sleeper may be roused by more powerful stimulants than those which preceded it,) becomes in its continuance, in consequence of the failure of those powers which formerly restored the sensitive system, complete.

As it is by the continued action of the vital parts in sleep that the sensitive parts are restored, the less active the former become, they necessarily effect their restoration the less readily; and when they can no longer effect it, the individual awakes no more; but the circumstance of the vital being no longer capable of restoring the sensitive system, makes no alteration in the nature of its exhaustion. It is still, while it lasts, the same exhaustion which constitutes sleep. The sleep proves final; but the sleeper is wholly unconscious of the cause which renders it so; and there is nothing in its commencement to inform us whether it will be final or not. Thus the sensibility is extinguished, and consequently respiration ceases. The extinction of the sensibility is the last act of dying, in the common acceptation of the term. As the ordinary stimulants of the day produce the sleep of daily occurrence, those of life produce the sleep of death.

Although the sleep of each day restores the sensitive system from the exhaustion which causes it, the daily recurrence of the exhaustion has the effect of permanently lessening the excitability of that system; a change not to be perceived from day to day, but which, from many phenomena, becomes sensible in the course of years. As the sensitive system becomes less excitable as the day advances than on first awaking, so it becomes less excitable as life advances than in childhood; and in like manner, as the repeated excitement of the sensitive system tends to the final decay of its sensibility, the continued excitement of the vital system, as we might *à priori* have supposed, has a similar tendency with respect to the excitability of this system. We find the pulse becoming slower as we advance in life, in consequence of the lessened excitability of the heart and blood-vessels, and the vital organs less readily influenced by the parts of the nervous system associated with them, proving that their functions also are under the process of decay. On the functions of these parts and the powers of circulation, all the assimilating processes depend; and the shrinking frames of the aged indicate their weakened state and the approach of their final extinction; for those were deceived who taught that there is nothing in the laws of our frame which should lead us to believe that it is not formed to last for ever.

The greatest degree of excitability, either in the sensitive or vital system, is not that which produces the most vigorous state of health. We may be too excitable as well as too little so. Many of the more serious diseases of children arise from this cause. The derangement of the digestive organs, which in the adult produces the nervous irritations of indigestion, produces in the infant inflammation of, and effusion on, the brain. The irritation of the gums, which produces pain and restlessness in the former, in the latter produces convulsions and death. Thus it is that the habit of the child is less firm and vigorous than that of the adult, which has acquired steadiness by the diminution of its excitability, in consequence of the continued action of the stimulants of life; but, after a certain period, the fault is a deficiency, not a redundance, of excitability, a defect apparently the necessary consequence of the laws of our frame, and to which every day unavoidably adds.

The redundance of excitability in children, the cause of many evils, we may be assured answers some important end. There is reason to believe that it is on it that the growth of the body depends, and that the due proportion between the excitability and the stimulants of life, by the gradual diminution of the former, determines the period at which the growth is completed in each individual. While the excitability continues redundant, the ordinary stimulants of life necessarily support a greater activity of the functions than is required for the mere maintenance of the body, and thus its volume enlarges, on the same principle that we have just seen it shrinks in the aged, in consequence of their excitability having become defective. It seems to be on this principle, namely, by a premature exhaustion of the excitability, that the hardships of life, that is, the greater than usual application of its stimulants, check the growth. On the same principle we should expect to find that the growth would cease soonest in the most excitable habits, because in them the excitability will soonest be reduced to a due balance with the stimulants of life. Thus it seems to be that the growth of women, who are more excitable than men, generally stops sooner, and consequently that they are of shorter stature, large women, for the most part, having less of the habit peculiar to their sex; and that by far the greater number of the most excitable men, who, in consequence of this constitution, make the greatest figure in their day, are men of short stature, while giants are generally of an opposite habit of body. There must, of course, to such rules be many exceptions. Where so many causes are operating, no result can be uniform.

THE form of death above described is the only one which, strictly speaking, can be regarded as natural. In all its other forms the regular course is disturbed by adventitious causes. But the causes which interfere with the regular course of nature, and which make their impression either directly on our bodies, or through the medium of our mental powers, are, in civilized society, so numerous and complicated, that it is rare to see an instance of such a death. At whatever period death arrives, it is almost always the effect of disease; and at advanced periods of life we only become more liable to death in consequence of our weakened powers rendering us more subject to disease.

Of the various instances of death I have witnessed, there was none that could be regarded as wholly the effect of age. It was always possible to point out some one or more of the vital organs more deranged than the rest, to which death was chiefly to be ascribed. We have, however, accounts of death from old age alone, which were such as has just been described, so that the inferences afforded by the laws of the animal economy are here confirmed by experience.

If we wish to prolong life, we must keep the attention so far directed to the health as to watch the first tendency to failure in any of the vital functions. In a great majority of instances, to a very late period of life, the failure in the commencement is capable of being corrected. By continuance it becomes obstinate, and by the laws

of sympathy spreads to other parts. We may be assured there is, in all, the capability of long life if they can escape the effects of disease. Thus it is that those who lead a quiet and retired life, little exposed to powerful impressions either of mind or body, often attain a great age. It is an additional motive for watching the state of health at advanced periods of life, that the longer we live the less in general is our suffering at the last; the nature of our death partaking the more of that of old age. For the further consideration of this subject I beg to refer to my Treatise *On the Preservation of Health, and particularly the Prevention of Organic Diseases.*

ALL modes of death, with the exception of that from old age, may be regarded as more or less violent; but in considering their nature, we must not confound the last act of dying with the suffering which precedes it, and which is often no less when it terminates in recovery than in death, which equally relieves it; and as death, in the usual acceptation of the word, from whatever cause it arises, consists in the loss of the sensorial functions alone, the act of dying is, in this respect, in all cases essentially the same. In all my experiments I found the nervous and muscular surviving the sensorial functions*.

When the animal no longer feels and wills, he is what we call dead; but for a certain time the motion of the blood in every part of the system still continues, and all the assimilating functions still go on, as may be demonstrated by dividing the vital nerves immediately after death, which produces the same change of structure in the organs supplied by them, though in a less degree, as during the life of the animal†; and that all this would be the case, a knowledge of the animal economy would have told us, independently of the aid of experiment, if we could, without this aid, have acquired it.

The removal of the sensorial powers neither destroys the muscular power nor deprives the muscles of involuntary motion of the stimulus which excites them. The heart, indeed, is incapable of its function, because, from the interruption of respiration, its left side is no longer supplied with the kind of blood which is its natural stimulant; and the accumulation of the blood in the lungs from the same cause affecting a great proportion of its vessels, prevents the right side from emptying itself. These are the necessary and almost immediate effects of the interruption of respiration; but the change in the blood of all the capillaries, with the exception of those which belong to this class of vessels, necessarily takes place more slowly. A certain time must always elapse before the stoppage of respiration greatly affects it. It has been sent to these vessels more or less in its proper state, and it still finds its vessels capable of being influenced by their usual stimulant‡. Thus, as I have ascertained

* Experimental Inquiry, Part II. chap. xi.

† Ibid. pp. 175, 176, compared with a paper which the Society did me the honour to publish in the Philosophical Transactions for 1827, entitled, *Some Observations on the Effects of dividing the Nerves of the Lungs, &c.*

‡ Philosophical Transactions for 1833.

by many experiments, the motion of the blood continues in these vessels for several hours after respiration has ceased, that is, as long as the blood can be drawn from the larger arteries,—the cause of these arteries being found empty some time after death*.

But this is not all; the nerves of the ganglionic, as well as cerebral system, retain their power for a certain time after the supply of that power from the brain and spinal marrow has ceased†. The blood therefore still finds the secreting surfaces in a state more or less capable of their functions, and the secreting processes, as I ascertained by frequently repeated experiments, still go on‡: nor is even this all, for the brain and spinal marrow depend for the continuance of their functions on the same powers as other organs; and I found, by an experiment made on so large a scale that it was impossible to be deceived in the result, that there is an actual supply of nervous influence after the sensorial functions have ceased, that is, after what is called death§.

SUCH is the natural decay of our frames; but, as I have already had occasion to observe, it is very rare for it to run its course uninterruptedly, particularly in civilized life. It is almost always disturbed by adventitious causes accelerating it, or the decay of particular parts, which, in consequence of the mutual dependence of the various functions, disorders the whole. Although these causes are of infinite variety, the laws of our frame are limited, and therefore many must operate on the same principle. This leads us to believe that, however varied the causes of disease, it may be possible to reduce their more ultimate effects to a few general heads. The exhaustion of the sensitive system, for example, is of the same nature, whatever be the cause of excitement; and other forms of debility, affecting either the sensitive or vital system, cannot be very various, however various the causes which produce them. We have reason to believe that the endless variety of disease depends more on the peculiar nature and functions of the different organs affected, and the peculiar manner in which different causes affect them, than on any great variety in the states which constitute the more immediate causes of death. However various the effects of disease, there must be but a few points to which they all tend, because the last in the chain of causes which produces what is called death, we shall find, is always the same, and seated in the same parts. On these principles we may hope to reduce the effects of the adventitious causes of death to a few heads, and thus to obtain such a view of the subject as shall enable us to trace the nature, and consequently the operation, of the causes of our decay in individual instances, and therefore to perceive more clearly the operation of the means which tend to counteract them. In the prosecution of the

* Experimental Inquiry, Part II. Experiments 66 and 67.

† See the Observations on the Experiments, which prove the evolution of caloric from the blood after what is called death, in the second part of the Inquiry just referred to.

‡ Ibid. Experiments 65, 69, 70.

§ Experimental Inquiry, Part II. Experiment 65.

subject I shall commence with those causes of disease whose operation most resembles that of the wholesome stimulants of life; and in pursuing, by means of the various experiments which tend to unfold the laws of the animal economy, the consequences of these causes, we shall be led to the effects of such as have nothing in common with them.

IT appears, from what was said of the nature of sleep, that all degrees of excitement in the parts of the brain and spinal marrow associated with the nerves of the sensitive system, are followed by proportional exhaustion. The only limit to this law is the capability of bearing in those parts. Exhausted by mental excitement, the criminal is often awakened for his execution; and the soldier, both by mental and bodily excitement, sleeps by the roaring cannon.

Now although the usual stimulants of the day never, except in old age, where we have seen all our powers have long been in a state of decay, produce such exhaustion as to endanger life, the exhaustion from stimulants of greater power cannot with safety be frequently repeated, because by their continued operation the sensitive parts of the brain and spinal marrow being both more exhausted than is consistent with the due state of the functions before sleep takes place, and roused before they have been refreshed to the usual degree by repose, a state of disease is induced; and all diseased states affecting the system generally, if their causes continue to operate, necessarily prove fatal.

Although in ordinary sleep the vital functions are for the time impaired in consequence of the lessened sensibility rendering the act of respiration less frequent, the state both of the vital and sensitive system is as much a state of health as in our waking hours. The insensibility of the latter only extends to the effects of the daily stimulants of life; and there are ample means in the functions of health for the restoration of this system, the powers of the vital system, as I have already had occasion to observe, being in no degree diminished, but only, in consequence of a slower respiration, less readily excited.

As soon as a diseased state of the sensitive system is established from the causes just mentioned, it begins to affect the vital system otherwise than through the intervention of respiration, the only medium, we have seen, through which the healthy exhaustion of the former affects the latter; for such is the sympathy between the sensitive and vital parts of the brain and spinal marrow, that any deviation from the healthy state of either is immediately felt by the other.

The characteristic of the mode of death I am considering, is the tendency of its causes to produce sleep in the first instance. So far their operation is the same, but greater in degree, with the common stimulants of life. At this period, if the cause of suffering be removed, the sleep is only more profound than on former occasions; and, as on them, it continues till the sensitive system again becomes obedient to those stimulants; if not, this system soon partakes of a species of debility so dif-

ferent from the healthy exhaustion, that instead of being relieved by the continued action of the vital parts of the brain and spinal marrow, it spreads to them. Hence the nutritive and other vital processes begin to fail*, and the various irritations which attend their failure, still further contribute to the debility of the sensitive system, and consequently, indirectly, to increase the cause of their failure. The derangement of each system thus aggravating that of the other, the evil proceeds not by simple addition, but in an increasing ratio, till all their powers are extinguished.

WHATEVER be the suffering which precedes what is called death, the moment of that death is but its termination, but the conclusion, as far as our feelings are concerned, of the process of dying. As soon as disease is established the act of dying is begun, and we have no reason to believe that, as far as the body is concerned, its nature is in any respect changed in what is called its termination. It is, from the first to the final ceasing of all the functions, a more rapid than natural decay of the powers of life, with, while sensibility lasts, more or less suffering, according to the cause which produces it. In recovery, our suffering terminates by the removal of that cause; in what is called death, by our becoming insensible to its effects; the bodily process being in no other way influenced by our total insensibility, to which the name of death is applied, but that the consequent ceasing of respiration accelerates it.

The body at this moment can no more be regarded as in the act of dying than at any other period of the disease; and the removal of the offending cause will not only in many cases at this period, if proper means be employed, but in some, even a short time after it, be followed by recovery. Thus, even after the period at which, according to the common meaning of the word, the process of dying is completed, it is, under certain circumstances, not too late to arrest that process, and restore the sufferer to the perfect enjoyment of his faculties. Recovery may take place after respiration has, from submersion, for a few minutes ceased, and the sufferer is, in the common acceptation of the term, dead, his sensibility, and consequently his respiration, independently of artificial means, being finally extinguished.

That this may happen, it is necessary not only that the vital system should have been just before in a state of healthful vigour, but also that the respiration should not have failed from the failing sensibility, but the operation of the offending cause. Here the sensibility fails from the failure of respiration, not, as in other cases, the respiration from the failure of the sensibility; but this difference in the succession of events makes no difference in the general nature of the actual state induced.

The recovery depends on our being able, more or less perfectly, to restore the function, the failure of which has caused the failure of all the others, as far as it has taken

* That the assimilating processes depend on the action of the nervous influence on the blood, appears from various experiments, an account of which has been laid before the Society. Many of these experiments are detailed at greater length, and others, illustrating the same position, added in my Inquiry into the Laws of the Vital Functions, Part II. chap. v., vii. and viii.

place, before the process of dying has proceeded too far for the restoration of the sensitive system. If no artificial means are employed, the date of death here is the time at which the sensibility ceased, and justly, because at that time death, according to the common meaning of the word, has taken place. The individual no longer feels and wills.

If there were even the last remains of sensibility, breathing would take place without external aid, as happens when the submersion has not been long enough wholly to extinguish it. The individual has, without such aid, finally ceased to feel and will, and is therefore what we call dead. His blood still continues to move, and all the assimilating processes, as appears from the experiments above referred to, are still going on; but this is no more than happens, more or less, in all cases after what is called death; the only difference being that from the nature of the offending cause, and the short duration of the disease, these functions are in a state of greater vigour than when the loss of respiration has been the effect of the loss of sensibility, which makes no difference in the nature either of their remaining powers or the circumstances in which they are placed, and would not prevent their ceasing, as usual, if no means were employed to arrest the dying process. I have dwelt the longer on this case, because it affords a good illustration of some of the preceding as well as following parts of the subject*.

THE approach of death, if we are aware of it, must always be more or less impressive, not only because we are about to undergo an unknown change, but are leaving all

* From the experiments which have been laid before the Society (Philosophical Transactions for 1822, 1827, and 1829, and Experimental Inquiry, Part II. chap. xii.), we have reason to believe that the effects of artificial respiration in restoring those whose breathing has been interrupted till the sensibility is destroyed, would be greatly aided by the use of voltaic electricity sent through the lungs in the direction of their nerves, and that many might thus be restored in whom inflation of the lungs alone fails. The inflation of the lungs in such cases acts in two ways. It gives to the blood of the smaller vessels of the lungs some of the arterial properties by which they are often excited, and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. le Gallois has shown that after the circulation has permanently ceased, the blood may, to a certain degree, be changed by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but even in the great arteries.

There is reason to believe, from the whole of my experiments, that the lungs should not be inflated more than eight or ten times in the minute, and that the injection of large quantities of air and great force in its injection should be avoided, and consequently the patient placed in the position in which the chest expands with greatest ease†. One of the chief defects of artificial breathing is, that in it the chest is expanded by

† Experiments relating to the effects of artificial respiration in the newly dead animal: Experimental Inquiry, Part II. chap. xii. If the air be thrown in more frequently or in greater quantity than the remaining powers of the lungs are capable of employing, it acts as a cooling process and is highly injurious. It is one of the defects of artificial respiration that we cannot tell either the precise quantity of air or the frequency of its injection required by the particular state of the circulating system in the lungs. We know that in the case before us, the demand cannot be equal to what it is in health.

that has hitherto interested and been grateful to us. Even here, however, for the most part, the laws of our nature are merciful. Most diseases of continuance, (for we shall find there are some exceptions,) not only gradually impair our sensibility, but alter our tastes. They not only render us less sensible to all impressions, but less capable of enjoying as far as we are still sensible to them. The sight of a feast to a man who has lost his appetite is disgustful, and a similar change takes place in a greater or less degree with respect to all other means of enjoyment.

These circumstances constitute a great part of the difference of our feelings with respect to what, in common language, is called a violent and a natural death. In the latter, as far as the sensibility is impaired, we are more or less in the state of old age, and, in addition to this change, our tastes are perverted. By these means the relish for life is in a great degree destroyed before we lose it. Thus in disease, the most timid often meet death with composure, and sometimes, as I have repeatedly witnessed, with pleasure. I have even known the information that the danger was passed, received only with expressions of regret.

To the form of death I am considering, belong a large proportion of the diseases of long standing, and whatever else tends gradually to exhaust the powers of the sensitive system, great mental excitement, too laborious a life, &c. The diseased state of the sensitive parts of the brain and spinal marrow, thus induced, spreading to the vital parts of those organs, terminates in a state of nervous apoplexy, the nature of which I had occasion to explain in the paper on Sleep above referred to, and to contrast with that of apoplexy from compression, in the most unmixed cases of which, the offending cause

the pressure of the injected air, whereas in natural breathing the air enters in consequence of its expansion. But the most essential difference between natural and artificial breathing in such circumstances is, that there cannot, till recovery is far advanced, be the proper supply of nervous influence, the due action of the vital parts of the brain and spinal marrow only being restored in proportion as the due force of circulation returns. Now it appears from what is said in the Philosophical Transactions for 1822 and 1827, and more fully in my Inquiry into the Laws of the Vital Functions, that voltaic electricity sent through the lungs in the direction of their nerves, is capable of performing as perfectly as that influence itself, the part which belongs to it in respiration, which is so essential, that the more perfect animal always dies from impeded respiration if the nervous influence be withdrawn from the lungs, unless voltaic electricity be supplied, which enables it to breathe as well as when the nervous influence is entire.

A proper apparatus, therefore, for sending voltaic electricity through the lungs in the direction of their nerves and in due power, should be added to the other means of resuscitation, which would render them, and probably to a great degree, more successful. The force of this observation will be perceived when it is considered that it is at the time of the first application of the remedies that the chance of recovery is greatest, and consequently that the immediate application of the whole means of healthy respiration, as far as we possess them, is of most consequence. It appears from what has been said, that the due functions of respiration cannot be restored till the due degree of nervous influence is supplied, and this cannot happen from inflation of the lungs till the due force of circulation returns. Now the fact, explain it as we may, is, that voltaic electricity so perfectly supplies the place of the nervous influence in the lungs, that their functions are equally perfect under the influence of either. The one can only be supplied at an advanced period of recovery, that is, in fact, only in those cases where the success of our endeavours can be secured by other means; the other is, in all cases, within our reach on the instant.

only producing a state analogous to the healthy exhaustion of the sensitive system but greater in degree, its influence is throughout confined to that system*. In the former case we see all the vital functions deranged; in the latter, the breathing alone affected, except as far as its state affects the others, death arising merely from respiration ceasing in consequence of the loss of sensibility; and so exclusively is this sometimes the case, that I had occasion to refer to an instance in which the patient breathed only two or three times in the last ten minutes, but each time drew the air freely into the lungs; a proof that he died without any accumulation of phlegm there, and consequently without any disorder of the vital functions, but such as arose from the increasing insensibility†. Here the failing powers of the sensitive affected the vital system in no other way than in sleep, the only difference being the degree in which the sensibility was impaired. Such cases are extremely rare. In by far the majority, from some inequality in the effects, or other peculiarity of the cause of pressure, at the same time that the sensibility is morbidly impaired, either a diseased state of a different kind is induced on the sensitive parts of the brain, which, as soon as established, begins to spread to the vital parts of that organ, or the cause of the disease itself more immediately affects the latter.

In the more rapid cases, the diseased state of the sensitive, which spreads to the vital parts of the brain and spinal marrow, supervenes without being preceded by a state of exhaustion, only differing from sleep in being greater in degree, in proportion as the stimulants which produce it are more powerful and protracted.

The effects of diseased states of the sensitive on the vital parts of the brain and spinal marrow, differ according to the nature and degree of the offending cause. When they are such as in the first instance to produce a state analogous to sleep, their injurious effects are necessarily more or less gradual, the first operation of the agent differing only in degree from that of the usual stimulants of life; but where the offending cause is more powerful in degree, or of a more injurious nature, the stage of exhaustion is lost, and the immediate effect on the sensitive system is that species of debility which the vital parts of the brain and spinal marrow having no power to relieve, partake

* It appears from experiments related in my Inquiry into the Laws of the Vital Functions, that simple and uniform pressure of the brain does not produce such a state of the vital parts of that organ as to derange the circulation, the effect of such pressure on the sensitive organs of the brain being of the same nature, as far as relates to the vital system, as the exhaustion occasioned by the exercise of their functions; which further appears from the whole functions of health being immediately restored on the removal of the pressure, which only proves fatal by its continuance more and more impairing, and at length destroying, the sensibility. (Experimental Inquiry, Part II. Experiment 18.) Many years ago, a man in whom the ossification of the skull had never been completed, exhibited himself in this country. By pressure made on the unossified part he was immediately brought into a state of apoplexy, which always disappeared, leaving him wholly uninjured, on the removal of the pressure.

† It has been shown by many experiments, detailed in the Philosophical Transactions, and in the second part of my Inquiry into the Laws of the Vital Functions, that derangement of the assimilating functions is always attended with accumulation of phlegm in the lungs, this being the first indication of derangement of these functions in them.

of; and when the cause is both violent and sudden, its effects on these parts are often such as immediately to destroy the circulation.

The Experiments, an account of which the Society did me the honour to publish in two papers in the Philosophical Transactions for 1815, prove that, although the heart and vessels do not derive their power from the brain and spinal marrow, it may be destroyed by impressions made on them. Thus it is that violent passions, either of a pleasurable or painful nature, in consequence of the sympathy which subsists between the sensitive and vital parts of these organs, have sometimes proved instantly fatal.

Here we have an effect from the causes of disease wholly different from that of the usual stimulants of life. The direct operation of the agent produces a state of debility in the sensitive system altogether of a different nature from that which constitutes the healthy exhaustion of sleep; and it will assist the memory and facilitate the means of reference to regard as the second form of what, for the sake of distinction, I call violent death, that which arises from all those causes which produce in the sensitive system this species of debility in the first instance, that is, debility without previous excitement, in whatever degree they have this effect; regarding, as the first species of such a death, the form of death we have been considering, that in which the cause, in the first instance, produces the stimulant effect, and consequently the exhaustion of sleep.

WHEN the cause of the second form of violent death, according to this division of the subject, is extreme, no time is afforded for its less powerful effects to show themselves. When it is less violent, so that the circulation, though impaired, still goes on, we find all the vital functions impaired along with it. The assimilating processes are doubly assailed by the failing supply of nervous influence and the lessened powers of circulation*. These effects, we have seen, may arise from the excess of the stimulant operation of agents†, but they are not necessarily the consequence of any operation of this kind, but may be as much the direct effect of the agent as the stimulant effect itself. It is, the offending cause and state of body being the same, when the operation of that cause is most powerful, that its debilitating effect is most unmixed. In proportion as it is less powerful, the case partakes more of the nature of the form of death, in which the first effect of the offending cause is that of a stimulant.

This is readily explained. I have been at much pains, in my Inquiry into the Laws of the Vital Functions, to point out that all agents capable of affecting the living animal, whether making their first impression on the mind or body, applied in a certain degree, act as stimulants, in a greater degree, as sedatives; that is, as means of directly impairing the power of the part they act upon‡. We know of no exception

* See note in p. 177. † See pp. 176 and 177.

‡ Experimental Inquiry, Part II., the last ten pages of chap. xi.; and the observations on the term sedative in my Treatise on the Influence of minute Doses of Mercury, which, from the want of some more appropriate term, I shall here employ for all agents which impair the power of the whole or any part of the animal frame without producing previous excitement.

to this law, and the stimulant and sedative effect of different agents bear no particular proportion to each other; but the greater the stimulant power of the agent, it must be applied to the greater extent to produce the sedative effect, and the greater its sedative power, in the smaller extent, to obtain from it the stimulant effect. The proportion which the stimulant and sedative effects of the same agent bear to each other is always the same, that is, its mode of application and the state of the body being the same; for the more gradual the application, the more the stimulant; the more sudden, the more the sedative effect prevails; and the less vigorous the functions, the less they are capable of the stimulant, and the more they are subject to the sedative effect. Thus torture, which, in the hardy savage, produces sleep, that is, the exhaustion which is the effect of the stimulant operation, acts as a sedative in the less robust European. While the former sleeps, the latter dies; and the more sudden its application the less the constitution is capable of resisting it.

The sedative effect, in whatever degree, is of a nature so different from the exhaustion which constitutes sleep, that its tendency always is to prevent the latter; and when the stimulant operation of the causes of disease exceeds that of the usual stimulants of life, and thus tends to the sedative effect, in the same proportion the tendency of these causes, although in the first instance to produce sleep proportioned to their stimulant effect, is eventually to prevent it. The repetition of fatigue at length produces fever, not sleep.

Such being the principles on which all agents capable of affecting the living animal operate, we readily perceive why the more sudden and powerful the cause of disease, the more it inclines directly to produce a state of debility, and when it is most so, why this tendency is unmixed with any degree of the stimulant effect.

But it is not necessary, as appears from what is said of nervous apoplexy in the preceding paper, that the operation of the agent should be either violent or sudden, to produce, even in the first instance, more or less of the sedative effect, if it be of a nature suited to produce it. In proportion as its application is less powerful, however, its peculiar effects are necessarily so also. Instead of preventing the tendency to sleep, it only impairs it; and the morbid state of the brain and spinal marrow shows itself by symptoms which less immediately threaten life. The sedative effect of agents may exist in all possible degrees, from the effect of the rage and joy which has produced instant death, to that of the settled grief, which only in the course of years destroys its victim; from the pain of a scald so extensive as to produce death in a few minutes, to the irritations of confirmed indigestion, under which the patient often lingers for a great portion of life. Whether the effects be sudden or gradual, the tendency, in all such cases, is the same, to terminate in a state of general debility, that is, nervous apoplexy, in which all the powers of the system are equally impaired.

The first impression of the cause is on the sensitive parts of our frame, which, without previous excitement proportioned to the debility which ensues, impairs their

functions; and this debility, in consequence of the sympathy which exists between the sensitive and vital parts of the brain and spinal marrow, spreads to the latter, and thus the vital functions are, more or less quickly, so impaired that they can no longer maintain those of the sensitive system.

The nature of this death is well illustrated by the effects of severe accidents, many of which operate on the same principle as the scald. The effects of severe blows on the head and spine are very complicated. They at once impress equally the sensitive and vital systems; but when the cause of injury is confined to less vital parts, as in the case of the scald, its first impression is on the sensitive system alone, or so nearly so, that the difference may be overlooked. Such was the cause of death in the case of the late Mr. Huskisson, with the circumstances of which the members of the Society are well acquainted; and hence it is that life is often saved by amputating a limb in which a cause of extreme irritation exists, that caused by the operation being more easily borne than the protracted irritation of a shattered limb, if the accident has not so subdued the strength that the additional irritation of the operation would prove immediately fatal.

To the same head belongs the death from the bite of rabid animals. The hydrophobia is a disease of the sensitive, spreading to the vital, parts of the brain and spinal marrow; and such is the effect of many other poisons.

It is evident that the form of death I am now considering is of the same nature as the preceding, with the exception of the early stage of the latter. The sedative state produced in the sensitive organs is of the same nature, whether it has arisen from the excess of the stimulant operation, or from the more direct effect of the agent, when applied in such extent as at once to produce this state. The symptoms produced in the sensitive, and the manner in which they influence the vital, system are the same in both. The same observations, therefore, which apply to the latter stage of the first of these forms, apply, more or less, to the whole progress of that we are considering. In both, what is called death is the final extinction of the sensibility; the termination, as far as relates to our consciousness, of the process which has been going on from the first establishment of the disease. As sleep is the completion of the temporary and limited exhaustion of the excitability which has been going on during the day, death is here the completion of its absolute and final exhaustion, which has been going on during the disease; and it is evident, that as the sensibility decreases, the suffering must become less, and consequently that it is least of all at the moment of what is called death. These observations, however, we shall find do not apply, in the same extent, to the forms of death which still remain to be considered.

THE three forms of death to which the attention has been directed in the preceding part of this paper, namely, that from old age, that from excessive stimulants acting on the sensitive parts of the brain and spinal marrow, and that from agents applied to such extent as to act as sedatives on those parts, agree in an essential respect.

The offending cause makes its impression on the organs of the sensitive system, and therefore in all, the sensibility is more or less directly impaired by it; and although it is only in the first that sleep can be regarded as the immediate cause of what is called death, the cause of injury in the second stage of the second form, and throughout the whole of the third form, producing the sedative effect, and consequently more or less tending to prevent sleep, yet tends, although in a different way, to impair the sensibility; and the termination in all such cases, as I have already had occasion to observe, if no other cause of injury arise in the course of the disease, is a state of nervous apoplexy, in so many cases the prelude of death, which, if not sufficiently violent or sudden, so to impair the powers of circulation as thus immediately to destroy those of the sensitive system, proves fatal by equally impairing the sensibility and impeding the assimilating processes; and as sleep relieves us from the ordinary stimulants of the day, the insensibility thus induced, relieves us from the sufferings of the disease, which, although it is not, like sleep, preceded by the grateful feelings of repose, is preceded by a gradual diminution of those sufferings.

THE forms of death which remain to be considered differ essentially from the foregoing. It will place in a clearer point of view both what I am about to say of these forms of it, and what has been said of its preceding forms, to consider more minutely than has hitherto been done in this paper, or, as far as I know, in any other discussion on the subject, the nature and relation of the functions of the living animal.

IN the community of functions which constitutes the life of man and all the more perfect animals, the sensitive are the working functions, those by which we perceive and act; the vital, those by which they are maintained. To the former, therefore, belong the immediate wear and tear of intercourse with the external world, and, consequently, the necessity of accommodating themselves to an infinite variety of circumstances. The vital functions, having but one object, pursue a steady course, from which, in health, they never deviate, except as far as is necessary to accommodate themselves to the necessities of the more eccentric functions of the sensitive system, the well-being of the organs of which depends on them; for they are capable of immediately influencing as well as being influenced by the inanimate agents which exist within our bodies; on the action of which the due structure as well as functions of every part depend. On this principle our food is digested; on the same principle the heart beats, and the secreting and other assimilating organs effect all their chemical changes. Thus the sensitive parts of the brain and spinal marrow are maintained, and thus also are maintained two sets of organs; through one of which, namely, the organs of the external senses with the nerves which convey the impressions made on them, these parts are capable of being influenced by the inanimate agents external to our bodies; and through the other of which, namely, the nerves and muscles of voluntary motion, they are capable of influencing those agents. These two sets of organs, allied by their

vital properties to the sensitive parts of the brain and spinal marrow, and by their capability of being excited by inanimate agents to the world which surrounds us, form the links which connect and enable to conduce to one end the operations of the sensitive organs, namely, the immediate organs of the sensorial powers, and the operations of inanimate nature; two classes of operations which have nothing in common. Let us here pause to consider more particularly the positions stated in this paragraph.

However repugnant it may be to our preconceived opinions, we shall, I think, when the whole of the facts on the subject are carefully weighed, find it impossible to avoid the conclusion, that all the vital functions, and all those functions of the sensitive system by which the sensorial powers influence and are influenced by the external world, are the results of inanimate agents acting on living parts, or living parts on them. Such, as far as I am capable of judging, must be the conclusion, if we compare the results of experiments, an account of which has been laid before the Society, and published in their Transactions *, with observations too simple to require any illustration from experiment.

With regard to the first of these classes, the vital functions, it is evident that the functions of the alimentary canal are excited by the food, of the lungs by the air, and of the heart and blood-vessels by the stimulating contents of the blood.

The blood, as it circulates in the vessels, is justly said to be alive. It possesses properties essentially different from those of inanimate matter; but we know that it is not by its vital properties, which are bestowed on it for other purposes, that it stimulates the heart and vessels, because its stimulating contents, when separated from it, produce the same effects on them. The experiments relating to the evolution of the caloric which supports animal temperature, point out one of the purposes answered by the vital properties of the blood †, and all the experiments relating to secretion and the other assimilating processes, point out the other purposes of its vitality. It possesses vital properties, not for the purpose of acting on other parts, but for that of duly responding to the inanimate agent, which acts on it in all these processes; for that the secreting and other assimilating processes depend on the action of an inanimate agent, appears from the experiments which prove that they depend on the nervous influence, which has been shown by direct experiment to be capable of its functions after it has been made to pass through other conductors than the nerves ‡, and cannot therefore have the properties of a vital power; to say nothing of those experiments by which it has been shown that all its functions may be performed by an agent which operates in inanimate nature §.

With regard to those functions by which the intercourse of the sensitive parts o

* Philosophical Transactions for 1817, 1822, 1827 and 1829; and Experimental Inquiry, Part. II. chap. xii.

† Experimental Inquiry, Part. II. Experiments 80, 81, 82, 83, 84, 85 and 86.

‡ Philosophical Transactions for 1822, 1829 and 1833; and Experimental Inquiry, Part. II. chap. xii.

§ Ibid.

the brain and spinal marrow with the external world is maintained, it is evident that the organs of the external senses are excited by inanimate agents external to our bodies, and that the muscles of voluntary motion are capable of influencing those agents; and we know that the impressions made on the external senses are propagated, and the muscles of voluntary motion excited, by the nerves, whose powers, as appears from the experiments just referred to, depend on an inanimate agent.

While the results of these experiments remain undisputed, if we assert that the nervous influence is a vital power, we must allow that such a power may exist in a mechanism wholly different from that to which it belongs in the living animal, and that all the functions of a living power may be performed by an agent which operates in inanimate nature; positions, which I believe no man, acquainted with the laws of the living animal, will be hardy enough to maintain.

Such, then, it would appear, is the nature of our frame. The sensitive parts of the brain and spinal marrow which are at once the immediate organs of enjoyment, the end of our being, and the source of those powers on which our intercourse with the external world depends, are maintained by a set of organs, the functions of which are excited by certain agents which belong to inanimate nature, and operate by other sets of organs which are capable of influencing, and being influenced by, every object around us, the functions of which are also excited by an agent of the same description. And these inferences are in no slight degree strengthened by another and distinct set of experiments, to which I referred in an early part of this paper, namely, those relating to the order in which the functions cease in the act of dying; for the whole of the phenomena traced by these experiments, as will more clearly appear from what I shall have occasion to say a little lower, tend to the same conclusions. Why do the nervous and muscular survive the sensorial functions? Why are the failing powers of life maintained in the organs of the two former classes of functions, after all trace of them is lost in the last class?

To the same conclusions, also, I cannot help thinking the following very simple train of reasoning might, without the aid of experiment, have led us. Although a single fact is often sufficient to establish the truth, when it is once arrived at, we almost always find others ready to give it their aid.

The phenomena of the three classes of functions above enumerated, namely, those by which our bodies are maintained, those by which the sensorial organs are influenced by the external world, and those by which they influence it, appear themselves sufficient to evince that the agents employed in their production partake of the nature of that world. Were not this the case, is it possible that the analogy between them and its phenomena could be such as we find it? Can we conceive a stronger analogy than the phenomena of inanimate nature bear to the propagation of an impulse along a nerve? Do not a thousand inanimate agents excite the muscular fibre in precisely the same way as the nervous influence does*? and it would be difficult to believe that

* See the first of my papers in the Philosophical Transactions for 1833.

the agent which operates in the formation of the secreted fluids from the blood and the other assimilating processes, is of a nature essentially different from that which effects similar changes in the laboratory of the chemist, even if the facts to which I have had occasion to refer had not been experimentally ascertained; but these facts, bearing more directly on the question, necessarily make a stronger impression.

Let us for a moment glance at those phenomena in which we are assured that no inanimate agent interferes. It is evident that the organs to which impressions made on the nerves are conveyed, must be those organs from which the nerves in question originate and derive their power. The sensitive nerves must communicate the impressions made on them to the sensitive parts of the brain and spinal marrow. It therefore follows that the sensorial functions, consequent on impression made on the nerves of the sensitive system, are the effects of the influence of the nerves on those parts of these organs. What are the results of this action of one vital part on another? Can we see any analogy between the phenomena of inanimate nature and pleasure or pain, the excitement of the feelings, or of the powers of reflection?

We thus readily perceive why the sensorial functions are the first which cease in dying. The stimulating parts of the blood are still present to excite the vessels, and the nervous influence, as appears from direct experiments above referred to, is still present to support the functions of the assimilating organs; but the sensorial functions being the results of vital parts acting on each other, as the vital powers fail, the powers of the parts acted on, and those which act upon them failing together, these functions necessarily cease. Here there is no inanimate agent present, as in the case of the nervous and muscular functions, to excite the languid powers of life*.

It is evident that in such a system as that I have been describing, there are two principles, either of which may determine the decay of all the sensitive functions. These, the functions by which the intercourse with the external world is maintained, may become incapable of their work, or those functions which maintain them, of their office. In the only natural death, that of old age, we have seen both these principles of decay in operation. The sensitive functions are gradually dimmed, and the vital functions gradually become less active.

Life, without much violence done to language, has been called a forced state. It consists of excitable parts called into action by suitable stimulants. These stimulants, it appears from what has been said, are all of an inanimate nature, for although the sensorial can only be excited through the nervous system, the action of the former, it

* It is observed in my Inquiry into the Laws of the Vital Functions, that in the most sudden death arising from causes which instantly destroy the powers of the nervous system, all the vital powers are at once destroyed; but this is only to be understood comparatively. The time in such cases required for their destruction is short; but in all the instances I have witnessed, the same succession, however rapid, could be observed. It was still evident that the muscular and nervous survived the sensorial functions. After the sensorial functions had ceased, slight flutterings of the heart and fleeting contractions of the muscles of voluntary motion could still be observed.

is evident, equally, though not so immediately, depends on the agents which excite the latter. Hence the harmony which exists between the living powers of the animal body and the powers of inanimate nature. There is nothing in common in the nature of these powers; but the organs of the former, being composed of the same materials with the world which surrounds us, can be excited by no means but the agents which operate in that world; and on what principle could we expect any other result?

These organs themselves are a part of inanimate nature. Deprived of their vital powers, they may still, as far as we see, be perfect in all their parts. On what their vital powers depend, we know not. In the study of these powers, and the relation they bear to the other powers of nature, we must be satisfied to take the facts as we find them. And what other knowledge have we of the inanimate powers themselves? Do we know more of the nature of gravitation or electricity than of life? It is the properties, not the essences, of things which are the objects of our senses. Our nature must be changed before the latter can be made a subject of inquiry. Life is a certain train of phenomena, depending on the peculiar state of its organs, produced by the action of the same agents, which operate in other parts of nature, on the material organs of our frame. We may arrange these phenomena in the way that best assists the memory, and best shows their relation to each other and the other phenomena of nature; but no task can be more hopeless than the attempt to proceed one step further, either with respect to the living powers or any other principle of action. Such an attempt is beyond not merely the limits, but the nature, of our minds. It is the blind attempting a knowledge of colours.

When we say we understand any of the phenomena of nature, we only mean that we are able to class them with other similar phenomena. We say that we know why a stone falls to the ground, because we class its fall with the other phenomena of gravitation. With regard to the phenomena of animal life, we at once see the limit of our inquiries, because it is self-evident that these phenomena exist nowhere but in the living animal, and consequently that there is no more general principle to which they can be referred; a position so evident that it is difficult to understand how it could ever have been overlooked.

It is customary to speak of life as a subject of peculiar mystery. But if what has just been said be correct, we have precisely the same means of acquaintance with it as with the other powers of nature. Its phenomena are as open to observation and experiment as the phenomena of any of these powers; and we possess no information respecting any of them but such as is derived from those sources. The greater appearance of mystery arises, not from the greater obscurity of the nature of life, but from its phenomena bearing less analogy to those of the other powers of nature than these bear to each other; in consequence of which the former are less familiar objects of contemplation. Simple as such observations are, they cannot be regarded as superfluous, when we see them overlooked by such writers as Hartley, Hunter, and others of almost equal name.

We cannot be surprised that the inanimate agents which are incapable of any change that unfits them for their office, should at length effect a permanent change in the vital parts on which they operate, of all parts of nature the most changeable. Hence the death of old age.

The sensorial functions we have seen fail first, because their organs are removed from the immediate action of the inanimate agents which still excite the organs to which they are directly applied; but for the same reason, it is in the latter, the organs of the nervous and muscular systems, that the decay begins. Their powers are gradually impaired by the operation of the inanimate agents which excite them, and the sensorial powers, as appears from all the phenomena of our decay, only fail in consequence of their failure; but as a certain vigour is necessary to render the latter capable of maintaining the sensorial functions, these necessarily cease before the total extinction of those which maintain them.

IN the forms of violent death which have been considered, the offending cause makes its impression on the organs of the sensitive, in those which remain to be considered, on the organs of the vital system.

IT is evident from what has been said of the nature and relations of the functions of the living animal, that there is one class of the causes of death which is necessarily confined to the vital organs. On them, we have seen, the inanimate agents on the operation of which life depends, make their impression. Those which impress the organs of the sensitive system excite only the functions by which our intercourse with the external world is maintained, and consequently may cease to operate without at all endangering life. But the withdrawal of the agents which excite the vital organs as certainly proves fatal as the loss of power in these organs themselves.

The operation of such causes is too simple to require any comment. It is evident that the want of food must destroy the digestive and other assimilating functions; that of air, the functions of the lungs; and the loss of blood, to a certain extent, those of the heart and blood-vessels.

THE other causes which belong to the forms of death I am now to consider, operate in a manner analogous to the offending causes which make their impression on the organs of the sensitive system; for although the vital organs are not subject to the same species of exhaustion with those of the sensitive system*, like them they may be debilitated either by the excess of the stimulant, or the more direct, effect of the agent, according to the degree in which it is applied. The excitement of fever terminates in debility of the heart and blood-vessels, or where the cause is more powerful, as we see in the worst forms of typhus, it may directly impair their powers;

* See my paper on Sleep in the Philosophical Transactions for 1833.

and similar observations apply to the effects of the offending cause on all the other vital organs. Although such are uniformly its effects on the parts on which it operates, its effects on the system in general, in consequence of the sympathies of our frame, admit of greater variety. These also may be divided into two classes.

In considering the second of the forms of death in which the impression of the offending cause is confined to the organs of the sensitive system, it appeared that when it is both violent and sudden, it immediately, in consequence of the sympathy of the sensitive and vital parts of the brain and spinal marrow, and the influence of the latter on the heart and blood-vessels, destroys the circulation*; whereas, when less powerful, it proves fatal, not only more slowly, but also in a different way. A similar observation applies to the causes of death which make their impression on the vital organs; for the circumstance of their being more or less violent and sudden, or making their impression on an organ more or less immediately essential to life, not only renders their effects more or less sudden, but essentially influences their nature.

When the cause affects an organ immediately essential to life, and is of such power as at once to destroy its function, death, depending wholly on the loss of that function, may be instantaneous; but when the cause operates less rapidly, or affects organs less immediately essential to life, death is not only more protracted, but the various causes of continued irritation which attend derangement of the vital, influencing the state of the sensitive system, it often arises as much from the impression made indirectly on the organs of this system, as on those to which the cause is applied, and sometimes more so. Thus, any cause which suddenly destroys the function of the heart or lungs, at once proves fatal, and the cause of death is simply the loss of a function immediately essential to life; but a loss of function in the intestines produces, not immediate death, but a series of causes of irritation, which exhaust the powers of the sensitive system, and death arises as much from this cause as from loss of function in the seat of the injury. Thus a blow on the stomach may instantly prove fatal by the impression it makes on the vital parts of the brain and spinal marrow without producing any other cause of derangement†; but inflammation of that organ, by the torture it occasions, often exhausts the powers of the sensitive system, before the inflammation has time to run the course that would prove fatal by its effects on the stomach itself.

We observe the same thing in a more remarkable degree where the organ is still less immediately essential to life, and the disease consequently is more protracted. It is in this way that stone in the bladder proves fatal. If such local mischief do not occur as disturbs the usual course of the disease, life terminates in the same way as from torture, only more slowly as the suffering is less severe and continued, that is, in a morbid debility of the powers of the sensitive system, more or less, according to circumstances, affecting the vital parts of the brain and spinal marrow, and the last

* Philosophical Transactions for 1815; and Experimental Inquiry, Part II. chap. ii.

† Ibid.

symptoms, as in cases where the cause of the disease makes its first impression on the sensitive organs themselves, are those of nervous apoplexy.

In this way death from causes of injury, making their impression on the vital organs, often approaches very nearly to the nature of the other forms of death which have been considered; and in almost all instances, with the exception of the most sudden, this is more or less the case; and consequently many of the observations made respecting the other forms of death, apply to the form I am now considering, particularly those relating to the gradual diminution of sensibility and perversion of taste which so generally precede, and more or less reconcile us to death.

I have already had occasion to observe, that even in some protracted cases there is little of this tendency. This, of course, is most apt to happen where the sensitive system is least affected, and therefore where the cause of injury makes its impression on vital organs of little sensibility,—on the lungs, for example, organs of peculiarly dull feeling,—a wise provision, for the air is so variously impregnated, and in so many ways which it is impossible to guard against, that were their sensibility acute, we should be exposed to constant causes of irritation. It is probably from its being so little so that, of all our organs, their sensibility is least apt to be increased by disease, the common effect of continued irritation. Those who have been troubled with carious teeth know how sensible the gums, parts of comparatively dull feeling, often become in disease. Even the most severe inflammation of the lungs may exist without pain, although the difficulty of breathing, cough and fever, which attend it, sometimes exhaust the feelings as much as pain. In its more chronic forms, however, it is often but little distressing even in these ways; and I have seen a few cases of pulmonary consumption, in which the sensibility and relish of life continued so entire, long after the patient was sensible of his approaching end, as to produce a state of mind peculiarly distressing, differing but little from that of those who look forward to what is called a violent death. This, however, is rare. In all serious and particularly tedious illness there is generally sufficient bodily suffering and perversion of taste, more or less, to blunt the sensibility, and in some measure to wean the patient from the love of life; and we generally find the grief and agitation on the part of the relatives, and on that of the patient, a degree of indifference and composure, which those who have only experienced the feelings of healthful vigour are at a loss to comprehend. Even the dread of death at length prepares us for it. The feelings of the criminal who is hanged on the instant are those of horror; of him who has languished in prison, of resignation.

But of whatever kind and degree the previous suffering may be, and by whatever cause produced, the last act of dying, in the common sense of the word, is still but the extinction of the sensibility, and consequently the termination of all suffering; and, as might from its nature have been foretold, so calm in general is this last act, that the most anxious observer often finds it impossible to ascertain the moment at which it takes place.

The circumstance which has given rise to our notions respecting the sufferings of our last moments is, that in certain diseases there is a convulsive action of the muscles at the time at which the sensibility is extinguished. But these are not acts of volition. The laws of our nature tell us that they are not the effects of suffering; and we never see in the patient any indication that he suffers. They are of the same nature with the convulsive motions of the epileptic, of which he is wholly unconscious. Were they indications of a struggle of feeling, necessarily connected with the last act of dying, as has been supposed, they would be a constant symptom; whereas they only occur under certain circumstances of the constitution or the disease. One of the least painful of violent deaths is that from loss of blood; yet here this struggle very uniformly attends the last act of dying, according to the common acceptation of the term; and it is evident that here the sensibility, in consequence of the failure of circulation, is almost extinguished before this involuntary action of the muscles takes place*.

It is generally supposed that the struggle of the criminal after the drop falls is the measure of his sufferings. The most vigorous necessarily suffer most, because in them the sensibility is with most difficulty extinguished; but it is not uniformly in them that this struggle is greatest. We have reason to believe that it is little, if at all, connected with the feelings of the sufferer. All such convulsive motions are of the same nature with what is called *subsultus tendinum*, so apt to occur in fever, even while the sensibility is little, if at all, impaired, but which gives no uneasiness but what arises from the motions of the limbs it occasions.

The causes of disease under various circumstances must act more or less interruptedly. In some cases their operation wholly ceases, and is renewed at intervals, causing the disease to intermit. There is a principle in the animal body on which the cure of all diseases depends, termed by writers the *vis medicatrix*, in consequence of which the more immediate effects of the offending cause are followed by others which tend to counteract them. If the surface of the bowels, for example, be irritated, a more copious secretion of their fluids and an increase of the peristaltic motion are excited, by which the irritation is relieved and the cause of injury expelled; and although there are few cases in which the operation of this power is so simple as in this instance, in all diseases its effects may more or less be observed, and a great part of the object of medical treatment, as far as the nature of the disease is understood, is to assist and

* It may appear at first view that our condition would have been improved had we not been endowed with the sensibility which often renders disease so great an evil; but in the same proportion as our ease would have thus been consulted, our danger would have been increased. It is by the quick sensibility of our frame that we are warned of a thousand dangers, and enabled to guard against them. Such is the imperfection of our present state, that we enjoy few advantages which have not occasionally their accompanying evils. But there is no instance but that of sleep, which is rather an imperfection than a positive evil, in which the evil necessarily exists; and thus we have reason to believe that the sum of enjoyment is the greatest of which that state admits. The species is protected at the expense of the individual.

regulate its operations*. We find even in those diseases which are of the most continued form, partly from its operation and partly from the cause of the disease acting more or less interruptedly, more or less evident remissions. Hence, and from a thousand accidental circumstances which influence the course of disease, and many of which it is impossible to trace, we find in diseases of continuance, that at one time the stimulant, at another the sedative effect prevails. Thus the sufferer appears at one time to be sinking, and at another to revive, without our always being able to trace the cause of such variations. All this the complicated nature of the animal body, and the various ways in which it may be influenced, would lead us to expect. We might also be led to expect that it would sometimes happen that when the excitability is nearly exhausted, such a cause of excitement might under certain circumstances occur as would suddenly exhaust that which still remains, and thus, by causing a sudden but temporary revival, prove the prelude to death. Hence what is termed a lightening before death, on which so many superstitions have been founded. This is seldom strongly marked. That it occasionally is so, we have sufficient evidence, and that it should be so, is perfectly consistent with the laws of the animal economy; but it will appear from what has been said, that, like the convulsive motions I have been considering, it has no essential connexion with the act of dying, and is not the consequence, but the cause, of its immediate approach.

Before I proceed to the last part of the subject, namely, the order in which the nervous and muscular functions cease, on which a very few remarks will be sufficient, I shall shortly recapitulate the leading features of the different forms of death, without recurring to the other parts of the subject, which are too numerous to admit of recapitulation; and make such additional observations as the recapitulation suggests.

WE have seen that the forms of death,—for, as I have already had occasion to observe, the whole operation of the causes of decay in strict language constitutes the act of dying,—may be arranged under five heads.

1. The only natural death, that from old age, where all the powers of life, in consequence of the operation of the agents which excite their organs, gradually decline, and death is only the last sleep, characterized by no peculiarity, in which these powers,

* Here, as in other instances, that imperfection of our present state, which we have reason to believe inseparable from it, appears. Nature, for example, relieves inflammation sometimes by exciting discharges from the inflamed part, sometimes by the process of suppuration; but she still employs the same means, although the effusion or suppuration by which the inflammation is relieved, from the nature or situation of the part affected, generally proves fatal. Such is the case in croup, the disease termed internal water of the head, inflammation of many vital organs, &c. In these cases it is the object of the physician to cure the inflammation by artificial means before it has time to run to such terminations. In other instances, as in some external inflammations, his object is to promote these operations of the *vis medicatrix*, as the least injurious way of removing the disease.

partly from their own decay, and partly from the lessened sensibility increasing the difficulty of restoring the sensitive system, become incapable of this office, in consequence of which the individual awakes no more; for it is to be recollected that it is not in the commencement, but in the progress of the last sleep that what we call death takes place. In its commencement, we have seen, the sleeper may always be roused by stronger stimulants than those which preceded it.

All the other forms of death, it appears from what has been said, may be regarded as more or less violent, some adventitious cause disturbing the natural process. They were divided into two classes; in the one the offending cause makes its impression on the sensitive, in the other, on the vital organs. The former were divided into those cases in which the debility which precedes the total loss of sensibility, arises from the excess of the stimulant operation of the offending cause, and those in which it is the direct effect of that cause; the latter into those causes in which the vital powers fail in consequence of their organs being deprived of the stimulants which excite them, and those in which the offending cause makes its impression on these organs themselves, the power of which, analogous to the operation of the offending cause on the sensitive organs, is destroyed, either by the excess of its stimulant, or its more directly debilitating operation, according to the nature or degree of that cause. Thus are induced,

2. The death which in its nature most nearly resembles the death of old age, that from excessive exhaustion of the sensitive system from the operation of stimulants of greater power than this system can bear, notwithstanding the intervals of such imperfect repose as their continued operation admits of, without the supervention of disease; which, not being capable of relief from the continued action of the vital parts of the brain and spinal marrow, by sympathy spreads to them, the affection of each system increasing that of the other, till all the powers of the sensitive system are destroyed.

3. The death in which disease of the sensitive system arises, not from causes overexciting, but directly debilitating it; the debility they produce, being of the same nature with that from excessive excitement, and running the same course as in the second stage of the preceding form.

4. The death which arises from the privation of the natural stimulants of the organs of life; and lastly,

5. That which arises from diseased states of those organs, analogous to the states produced in the organs of the sensitive system by the causes which make their impression on them.

IF the foregoing include all the modes of decay, the physiological nature of death in its various forms is referable to very simple principles. In the natural decay the excitability of the organs of both the sensitive and vital systems is gradually impaired by stimulants, which, whether existing within our bodies or making their impression from without, belong to inanimate nature; for it is by the impression of such stimu-

lants alone that the functions of life are maintained. In the different kinds of violent death, with the exception of the death which arises from a failure of the natural stimulants of the vital organs, which is comparatively rare and extremely simple in its nature, we find the excitability of one or both of these systems, or some parts of one or both of them, capable of influencing all the others, more quickly destroyed by the continued operation of causes which either stimulate beyond the limits of health, or, applied beyond the limits of their stimulant operation, destroy the powers of life, either by directly destroying the powers of the sensitive system or depriving it of those powers by which it is maintained. All these causes, it is evident, tend to the same effect, the extinction of the sensibility, which constitutes death according to the common acceptation of the term, the immediate cause of which, therefore, exists in the sensitive parts of the brain and spinal marrow.

Thus it appears that, in every instance,—for it will be found, I believe, that there is no case of death which may not be referred to one of the foregoing heads,—what is called death and the loss of sensibility are one and the same, and therefore that the last act of dying can in no instance be an act of suffering; and this we have seen confirmed by direct observation, as far as the observation of the bystander can confirm it; to which may be added the experience of the sufferer himself, because those who, from submersion or other similar causes, have passed that portion of the act of dying where suffering can alone take place, and who have, as above explained, been in the common sense of the word dead, and in consequence of the degree of vigour still remaining in the vital organs restored by inflating the lungs, declare that they had been sensible of no suffering but such as arises from a less degree of the same cause which in them had wholly extinguished sensibility; an observation well illustrated by the circumstance, that those who are restored by artificial respiration, and could not have returned to life without this aid, and those whose breathing, not having been long enough suspended wholly to destroy the sensibility, and who consequently, although to all appearance equally insensible, in a short time after the cause is removed, breathe spontaneously, give precisely the same account of their sufferings.

In those in whom the sensibility has been extinguished by submersion, it is in the first part of the process by which they recover, not in the last part of that by which they lose it, that they suffer, which it is not difficult to explain.

In the latter the sensibility is almost lost before it is wholly so. The apoplectic who has still feeling enough to breathe, who may still be roused to remove the extreme cause of suffering which the want of a supply of air in the lungs occasions, may be insensible to all other causes of excitement; for in proportion to the immediate importance of that supply, is the feeling which impels us to obtain it. We have instances of the hand being voluntarily held in the fire; but none, of the breathing voluntarily stopped till the lungs were injured. The circumstance of the breathing, independently of artificial means, being finally lost, is a proof that the sensibility is wholly extinguished; and as its extinction in such a case must be more or less gradual, the

capability of acute suffering, it is evident, must be lost some time before the period at which the want of air in the lungs cannot even be felt.

In the act of recovery, on the other hand, the sensibility necessarily begins to revive before the vital organs perfectly recover their functions after so severe a shock. The sensitive, on its revival, thus finds the vital system still more or less in a state of disease, to which the former, as its powers increase, is every moment becoming more sensible; for while the powers of both remain, all derangement of the vital is felt by the sensitive system; a wise provision, by which we are warned to guard against causes of danger confined to the former.

IT will readily occur from what has been said, to those whom I have the honour to address, that under certain circumstances more than one of the preceding forms of death may concur. The first indeed, the death of old age, may be regarded as so far a combination of more than one of the other forms, that the cause makes its impression on both the sensitive and vital systems; but its effects on both, as appears from all that has been said, are essentially different from those of disease.

In certain cases the cause of disease makes its impression on both systems, and then more than one of the last four forms concur. This, I have already had occasion to point out, necessarily happens from mechanical injury of considerable portions either of the brain or spinal marrow. When both systems are directly impressed by the cause of the disease, which is comparatively rare, it produces, as follows from what has been said, a combination of the third and fifth, or second and fifth forms, according as its effects are more or less sudden and severe.

SUCH in different cases, is the varied course of our decay previous to the moment at which the sensibility is extinguished, emphatically called that of death, because it completes the decay of the sensorial powers, and leaves us only those which we possess in common with the vegetable world; for the vegetable, like the animal, can convey its juices, form its secreted fluids, and in some instances move its limbs, if proper stimulants be applied; an additional argument, it might be shown, if any were required, for all such functions being the effects of inanimate agents acting on living parts.

After the removal of the sensorial functions, none remain to us but such as are maintained by the immediate action of those agents. Our bodies are hastening to be mingled with the matter of inanimate nature. They retain only those powers which immediately depend on its agents, and these are rapidly failing, because, for reasons which have been pointed out at length*, the due application of those agents in the more perfect animals cannot long survive the loss of the sensorial powers.

The power of organizing the elements of inanimate nature belongs, and some have

* Philosophical Transactions for 1829; and Experimental Inquiry, Part II. chap. xi.

supposed exclusively, to the vegetable world; but as we see plants, the mushroom tribe, possessed of no organizing power, and therefore, like animals, nourished only by matter already organized, some of the lower species of animals, on the other hand, seem to possess this power. Thus, it would appear that there is a class of animals and of plants in which the animal and vegetable, in this essential respect, exchange their natures. As the animal becomes imperfect, and approaches the nature of the vegetable, the sensorial powers dwindle, and the lowest animals appear to extract their nutriment from air and water, which, being generally diffused, are at hand, and consequently obtained without any sensible effort on the part of the animal. His life, therefore, although not independent of the external world, is, like that of the vegetable, independent of any act of volition. As we rise in the scale of animals, the sensorial powers increase, and, in the same proportion, become more essential to existence. From those animals which obtain food without any act of volition, we come to those who can only obtain it by such an act, but who still without any act of this kind obtain the influence of the air, yet more immediately necessary to their existence. We arrive at length at the most perfect class, which can neither obtain food nor air, except by an act of the sensorium. In them the sensorial power is as necessary for the inhalation of the air, as the ingestion of the food. When sensation ceases, they as certainly cease to breathe as they cease to eat. Thus it is that in this class of animals the due application of the inanimate agents on which life depends, cannot long survive the loss of the sensorial functions.

AS we have been enabled, by the aid of the experiments referred to in the foregoing paper, to trace the steps by which the sensibility in the various forms of death is extinguished, that is, of our decay up to that moment which has for very evident reasons obtained the name of death, by the same means we may with more ease trace the steps by which the remaining powers of life are extinguished.

AS the powers of life fail, we have seen, the first functions which cease are those which wholly depend on these powers. The others, being the results of inanimate agents acting on vital parts, continue as long as those agents are supplied, for the purpose of exciting their organs. The first of these powers which fails is evidently the power of the capillary vessels, because their function continues as long as any blood can be supplied to them from the larger arteries. The circumstance of the action of the capillaries only ceasing when the larger arteries are empty, affords a proof that the assimilating processes, without which their power would fail, are still more or less in a state of activity. These processes, we have seen, are immediately dependent on the vital parts of the brain and spinal marrow. The due mechanism of every part, it appears from direct experiment, depends on the action on the blood of the agent they supply. When the capillaries can no longer supply the blood on which it acts, it is evident that the functions of this agent must cease, and conse-

quently that those parts of the brain and spinal marrow, by which it is supplied, being thus deranged, their powers must cease also*. These are the last of the powers of life which fail, and thus the body of the more perfect animal is left subject to the laws of inanimate matter. The first functions which cease are those of the sensitive parts of the brain and spinal marrow; the last, those of the vital parts of these organs.

* In the first of any of the more perfect animals, the nervous influence must have been supplied from without, or the rudiments of the organs which supply it and those of the sanguiferous system must have been simultaneous creations, because neither is capable of producing the other, the functions of each being inseparable from those of the other. We have seen that it is a necessary inference from direct experiment, that while the vital principle is unimpaired, the powers of circulation, provided the blood be duly exposed to the influence of the air, are capable, with the aid of voltaic electricity, of all the assimilating functions. No other powers are required for the maintenance and growth of the animal body.

We have reason to believe that the vital parts of the brain and spinal marrow may, like the lungs, be inactive in the fœtal state, some other means in this state being employed to supply the agent which, after birth, can only be supplied by them. Well grown fœtuses, perfect in all their other parts, have been born without either brain or spinal marrow. The growth of such fœtuses must depend on the same causes as the growth of other monstrous productions in the uterus, namely, as far as relates to the brain and spinal marrow, on the powers of the mother alone, how applied it is impossible to say.

XI. *An Account of a Concave Achromatic Glass Lens, as adapted to the Wired Micrometer when applied to a Telescope, which has the property of increasing the magnifying power of the Telescope without increasing the diameter of the Micrometer Wires.* *By* GEORGE DOLLOND, *F.R.S. &c.*

Received February 19,—Read February 27, 1834.

WHEN the application of any optical or other arrangement is found to be useful, a correct statement of the manner in which it became so is essentially requisite, in order that each person who may have had a share in bringing it forward may have his due proportion of the merit.

The achromatic lens which I have applied to the wired micrometer, and which has been found to produce such very considerable advantages to that instrument, arose out of a trial that was made at the suggestion of Professor BARLOW, for the purpose of improving the chromatic aberrations which affected the field of the eye-glasses applied to the telescope invented by that gentleman with a fluid correcting lens, and made by myself for the Royal Society.

The lens in question not being found so effective for his purpose as he expected, was laid aside. It has now been introduced for my purpose, and is made, with some trifling variations, in accordance with his calculations.

The interposition of a concave lens between the object-glass and the eye-glass of a telescope has been generally known by opticians to produce an increase of the magnifying power, in proportion to its focal length and distance from the object-glass: also that a convex lens, if so applied, would diminish the power.

Except in the Huygenian eye-tube, I am not aware that either of these lenses have been so applied generally, it having been considered that their introduction would materially diminish the light proceeding from the object-glass of the telescope, and also, by deranging the aberrations, disturb the image.

In the lens I am now describing, these errors are very materially obviated, owing to its being constructed upon achromatic principles*, by which the magnifying power of the telescope is increased in a twofold ratio, without so much diminution of light as is produced by the introduction of a simple lens.

For example, if the eye-glasses in the original arrangement of the telescope gave 100 of magnifying power, the same eye-glasses with the new lens, if I may so term it, will give 200, and the light will be fully equal to that power if obtained by the usual means. The field of view will also be considerably flattened.

* The discovery of JOHN DOLLOND, F.R.S., in the year 1758.

Thus it will be seen that we have the advantage of using longer eye-glasses with an extension of power, whereby the wires or spiderwebs of the micrometer are not increased in diameter, a very essential advantage when observing minute double stars; nor is the eye of the observer so much distressed as when the magnifying power is obtained by shortening the focal lengths of the eye-glasses.

The advantages of this improvement having been shown by the foregoing introduction, I will now proceed to give an account of the causes which led to its being applied to the micrometer, and the result of its application.

The Rev. W. R. Dawes of Ormskirk, a gentleman pursuing practical astronomy with great zeal and perseverance, and to whom the public are already much indebted for several valuable communications, being desirous of carrying his measurements, &c., of the double and revolving stars, to a greater extent than the powers of his micrometer then allowed, applied to me to construct for him an arrangement of eye-glasses that would increase the magnifying power of his telescope without increasing the apparent diameter of the spiderwebs in his micrometer, or interfering with the mode of illumination. Several combinations were tried without success, when it occurred to me that the achromatic concave lens, which had been decided by Mr. Barlow to be of no use for his purpose, might accomplish what was required.

The result I will now state from a letter I soon after received from Mr. Dawes, to whose micrometer this improvement had been applied.

"Ormskirk, March 14, 1833.

"My dear Sir,—You will doubtless be surprised at not receiving from me any account of the performance of your scheme for the improvement of the achromatic telescope.

"My general opinion of your improvement is, that it is, for the purpose it is designed to answer, as useful as it is elegant.

"By a careful determination of the value of the micrometer divisions, I find the magnifying power of any eye-tube is increased in the proportion of 2·1068 to 1: each part originally = 0″·555922 is now = 0″·263867. To obtain the magnifying powers of the eye-tubes, I content myself with multiplying the original powers by 2·1. But I will detail a few particulars noted in my journal on the subject. I have thus set down the advantages of the additional lens.

"1st. The micrometer threads are only half the thickness, with the same magnifying power on the object; small stars are therefore neither obliterated nor distorted.

"2nd. The parallel threads are both very nearly in focus with any power up to 600; before, only up to 285 (the same eye-piece).

"3rd. The value of the micrometer divisions is less than one half its former amount, permitting a proportionally fine motion in measuring the distances of delicate objects.

"4th. A much greater extent of the field being flat, and the threads distinct further from the centre; of great importance in accurately determining the zero of position by the passage of a star along the thread.

"5th. The definition of the stars seems quite as good; and the false light does not appear to be increased, or the regularity of its distribution affected. The discs of the stars seem in fact to be, if anything, rather smaller and cleaner with the concave. Perhaps their brightness might be perceived to be a trifle less; but even this is doubtful. See below.

"6th. The shallower eye-glasses are much more easily cleaned; of great importance in high powers.

"7th. The prism can be conveniently applied to all powers as high as 600; before, only to 285. This prism is of essential utility in other respects besides facilitating zenith observations; and it is no small improvement that its use is thus extended.

"From the performance of this additional lens, it is evidently a perfect production. Against all the advantages detailed above, the trifling addition to the length of the telescope is not to be mentioned; indeed it is to me surprising that so great an effect should be produced with so minute an increase of focus.

"As a severe trial of the difference in illuminating power, I have examined Saturn's satellites, and $\varkappa$ Geminorum. I could discover no decided difference in the apparent brightness of the satellites, allowance being made for the difference of power employed. It happens awkwardly, that among moderate powers, fit for planets, none coincide sufficiently with and without the concave lens. The nearest I can get are a negative 195 with the new lens, and a double convex 208 without it: with these, little difference in brightness; but the planet might be a trifle sharper with the latter. Have you ever seen the minute companion of $\varkappa$ Geminorum? It is the finest test of a five-feet achromatic I have yet seen: distance about 6″. I saw it steadily with negative 140 without the concave, and quite as well with negative 116 with it; but these powers are not near enough to each other.

"For tolerably bright stars, I have on the micrometer 475 with the concave lens, and without it 480; also 600 with, and 625 without. These afford an excellent comparison. Vision appears to me equally good with both; and the fineness of the micrometer threads leads me always to prefer the new arrangement, as I can then use the same eye-piece generally for the distances, as I use for the positions.

"In clear weather, I always use 600 for stars of the fifth magnitude and upwards, and sometimes even of the sixth; and last night I got a very good set of positions of Castor with a power of 1010, with which the discs were occasionally perfectly well formed, though of course not so sharply defined. I also obtained last night very satisfactory measures of ζ Cancri, certainly one of the most difficult stars for a telescope of five feet. That you may judge for yourself of the way in which it was seen, I will detail here my measures, exactly transcribed from my observation paper.

Power.	Position.	
600	336° 56′	Mean = 335° 28′
Stars placed between the parallel wires thus:	335 50	z = − 271 26
	336 22	
	335 8	64 2 nf.
	335 7	
	334 1	= 25 58 from N.
	334 49	

Power.	Distance. P. decl.		P. decl.	
295	+ 4·0	−	4·2	Mean of all = 4ᵖ·33, which = 1″·143.
	4·2		5·0	
	4·3		4·4	
	4·3		4·2	
	4·4		4·3	

N.B. The plus and minus measures are taken alternately, and not one rejected or altered.

"Though 600 did well for the angles, the stars were not sharp enough with that high power for accurate bisection. The parallel threads are sweetly fine and sharp with 295 (formerly 140). Indeed, this is a very efficient and generally useful power.

"Thus you will see, my dear Sir, that a long-lamented desideratum has been efficiently supplied by your elegant invention. I have thus nearly all the advantages, and none of the disadvantages, of a ten-feet telescope of the same aperture.

"I remain, my dear Sir,

"Yours faithfully,

(Signed) "W. R. Dawes."

I shall now introduce some extracts from a letter I have since received from Professor Barlow, in which his formulæ for constructing the lens are given.

"Woolwich, February 1st, 1834.

"Dear Sir,—In answer to your letter of January 30th, 1834, I will endeavour to state the views which led to my requesting you to make the achromatic concave lens you allude to, and explain the formulæ and principles on which I computed the curves.

"First, with regard to my views. Every one is aware of the ease and comfort of observing objects in a long telescope in comparison with viewing the same in a short one, supposing the powers equal in both instruments; and my object was to produce this effect by taking up the rays before they arrived at their focus, extending them to a greater distance, and thereby increasing the size of the image, which is of course the same as increasing the length of the telescope in a like proportion.

"In order to render this lens achromatic, it is only necessary to make the foci of the lenses proportional to their dispersive powers, as in the object-glass itself; except that here the crown lens must be made concave and the flint lens convex.

"Suppose, for example, the compound lens is to be placed at a distance, d, from the focus, and that the image is to be doubled, then the focal length of the compound lens must be $2d$; for $\frac{1}{d} - \frac{1}{2d} = \frac{1}{2d}$: again, δ being the dispersive ratio, we have

$$f = 2d(1-\delta) = \text{focal length of the crown lens,}$$

$$f' = \frac{2d(1-\delta)}{\delta} = \text{focal length of the flint lens.}$$

"To correct the spherical aberration requires more labour. Let us suppose the crown lens placed towards the object-glass. Assume its radii r, r', or rather their ratio $\frac{r}{r'} = q$,

at pleasure, and compute its aberration for rays converging to the distance d, which may be done by the following formulæ, a being the index.

"Find

$$d' = \frac{(a+1)}{a\,d - r}\,dr,\quad b = \frac{a}{a+1},\quad \frac{d}{r} = c,\quad \frac{d'}{r'} = c',\ \text{and}\ \frac{r}{r'} = q;$$

then the aberration will be

$$\text{aberration} = \left\{ \frac{(c+q)^3}{(a\,c-q)^3} \times \frac{c+(a+2)\,q}{c\,(a\,c'+a+1)^3} + \frac{(c'+1)^3}{(b\,c'+1)^3} \times \frac{(c'+2-b)\,q}{c'} \right\} \times \frac{a}{2\,r}\,*$$

"Let the quantity when found be called m, then for the flint lens proceed as below, the radii being r'', r''', the latter towards the eye, and the index a'.

"Find

$$d'' = \frac{(a'+1)\,2\,d\,r'''}{2\,a'\,d - r'''},\quad b = \frac{a'}{a'+1},\quad \frac{2\,d}{r'''} = c,\quad \frac{d''}{r'''} = c',\ \text{and}\ \frac{r''}{r'''} = q.$$

"Then find r''', r'' and q, such, that

$$\left\{ \frac{(c+q)^3}{(a'\,c-q)^3} \times \frac{c+(a+2)\,q}{c\,(a'\,c'+a'+1)^3} + \frac{(c'+1)^3}{(b\,c'+1)^3} \times \frac{(c'+2-b)\,q}{c'} \right\} \times \frac{a'}{2\,r'''} = m,$$

and the resulting curves will be those required.

"To produce this latter equality is the only difficulty in the operation, and to treat it as a common equation would lead to immense labour. I have therefore always contented myself with pursuing the more simple method of trial and error, its facility fully compensating, in my mind, for its want of scientific elegance.

"It may be proper to observe, that I proposed the lens to double the magnifying power, and the curves were computed accordingly, but the formulæ will of course apply to magnifying in any ratio.

"I hope this explanation will be found intelligible, and I am pleased to find my proposition has been found useful.

"I remain, dear Sir,

"Yours very truly,

(Signed) "Peter Barlow."

I have only to add to the foregoing relation of facts, that I do hope they will prove satisfactory to those friends who have felt so much interested upon the subject as to induce me to write this Paper, it not being my wish to take credit to myself for anything like an invention, but merely for the application of the lens to the micrometer, as I am fully convinced that a concave lens, either simple or achromatic, was never so applied before.

* See Philosophical Transactions, 1827, p. 244.

February 17*th*, 1834.

XII. *On the Principle of Construction and general Application of the Negative Achromatic Lens to Telescopes and Eyepieces of every description.* *By* PETER BARLOW, *Esq. F.R.S. &c.*

Received May 20,—Read May 29, 1834.

THE great advantage which has attended Mr. DOLLOND's ingenious application of the negative achromatic lens to the micrometer eyepiece, seems to make it desirable that the principles on which that lens is constructed, and its general application, should be more fully illustrated than is done in the short extract made from my letter to Mr. DOLLOND, and given by him in his recent paper in the Philosophical Transactions.

In my original fluid telescope, the negative lens was employed for the double purpose of lengthening out the focus and correcting the colour of the front lens; and the great advantage of the lengthening principle was manifested by the high penetrating power of the instrument in the centre of the field. Unfortunately, however, the perfect part of this was very limited, so that when Mr. DOLLOND constructed the second telescope for the Royal Society, I gave up this advantage for the sake of enlarging the field; but I found that by this means much of the penetrating power of the former telescope was lost; for although I had the same aperture, many small stars which were before very perspicuous were in this instrument seen only with difficulty and under advantageous circumstances of weather, absence of moonlight, &c.

This led me to consider whether it would not be possible to retain the advantages I had obtained in the new instrument, and to restore the power of the other principle (that of penetration) by an artificial lengthening of the focus; but as the rays were now as nearly achromatic as I could make them, it was necessary in this case to have the lengthening lens also achromatic. I had no authority from the Royal Society to make any collateral experiment, but having mentioned my idea to Mr. DOLLOND, he very readily undertook to construct the small lens, and it was accordingly made and tried; but owing, as I now imagine, to the imperfect means I had of fixing it, its advantages were not perceived. It was laid aside, was not referred to in my paper, and would most likely have been altogether lost sight of, had it not occurred again to Mr. DOLLOND to try its effect on the micrometer eyepiece for the Rev. Mr. DAWES. It is therefore to Mr. DOLLOND we are indebted for snatching this lens from the oblivion into which I had allowed it to fall.

It must not, however, be understood that it is only applicable to this eyepiece, for it may be applied to any eyepiece, positive or negative, or to the erecting eye-

piece, or indeed to any telescope of fluid or glass, or to refractors; for it is, in fact, not a part of the eyepiece, but of the telescope itself: and it is for this reason its advantages are so conspicuous in the application Mr. Dollond has so ingeniously made of it; for by lengthening the focus before the rays arrive at the eyepiece, the image is magnified, while the wires retain only their original size.

Having thus shown the origin of the negative achromatic lens, I may be allowed to state the motives and reasonings which guided me in the computation of the curves, and what appears to me to constitute the advantages it is found to possess. Notwithstanding the extreme difficulty there is in constructing an achromatic object-glass, yet with perfect materials the difficulty is only in the manipulation; and this being overcome, there is not so great a natural impediment to perfection in this part as in the eyepiece,—for we know that it is impossible to make a perfect positive power*; and if the same absolute impediment does not occur in the negative eyepiece, yet the thicknesses of the lenses render the task very difficult, not only to execute, but to compute the proper curvatures to ensure perfection. If this view of the case be correct, we see at once the advantage of magnifying the object as much as possible before we apply the eyepiece; and this, in fact, is the whole theory of the negative achromatic lens: that is, supposing the rays to be rendered achromatic by the object-glass, they are intercepted by the negative lens before they cross, which, being itself also achromatic, extends them to any length, and thereby produces the effect of lengthening the whole focus in the same proportion, and consequently the power of the telescope, the eyepiece remaining unaltered.

In the conclusion of my letter to Mr. Dollond, I have offered a suggestion, whether it would not be possible to retain the same eyepiece for all powers by changing only the negative lens. This must of course, as he has observed, change the scale of the micrometer; but this being changed, by so adapting the lens as to render the powers simple multiples of each other, would not, I conceive, be attended with any disadvantage. In other cases, where a micrometer is not employed, and where the utmost perfection is not looked for, every variety of power may be produced by simply moving the negative lens nearer to or further from the eyepiece; for both the object-glass and lengthening lens being achromatic, the image, wherever the focus is formed, will be achromatic also; and the spherical aberration of the lens is so inconsiderable, as only to be discovered by the most perfect eye, when removed from that point in which it is computed to be perfectly corrected. The negative lens is therefore admirably suited for day telescopes with correcting eyepieces, as also for astronomical telescopes where the micrometer is not applied; for by giving an adjustment to the lengthening lens, the power may be changed in any proportion, even without removing the eye or losing sight of the object. I have no doubt that these and other applications of the lengthening lens will be made, and amongst others, I am willing to hope that

* See Professor Airy on the Eyepieces of Telescopes, Transactions of the Cambridge Philosophical Society, vol. iii.

it is not impossible the negative secondary spectrum of this lens may, by careful experiment, be so proportioned as in part to counteract the positive secondary spectrum of the object-glass so as to render the image more nearly aplanatic; some experiments, at all events, directed to this inquiry are very desirable.

I have already, in my letter to Mr. Dollond, given the formulæ for computing the proper curves according to any distance between the focus and the lengthening lens, and for magnifying the image in any required proportion; but unfortunately the calculation is very laborious, and difficult to be rendered general, or tabulated for general practice. I would therefore recommend opticians to use the same curves as are commonly adopted for short telescopes of six, eight, or ten inches, making those of the plate or crown concave instead of convex, and those of the flint convex instead of concave, turning the plate towards the object-glass and the flint towards the eyepiece, which will in general bring out a close approximation for spherical aberration, and the colour will be sure to be corrected. Starting from this point, practical skill will readily supply the means of making corrections, if any such should be found necessary after all has been done that can be done by changing the position of the lens as regards its distance from the eyepiece. I hope these additional directions for constructing and applying the lengthening lens will not be thought superfluous, nor undeserving the attention of practical opticians.

XIII. *Some Suggestions relative to the best Method of employing the New Zenith Telescope lately erected at the Royal Observatory.* By JOHN POND, *Esq. A.R. F.R.S.*

Received March 11,—Read March 13, 1834.

THE erection of a zenith telescope of twenty-five feet focal length at the Greenwich Observatory was determined upon by the Visitors in the year 1815, for the purpose of measuring the zenith distance of γ Draconis with greater accuracy than could be effected by any instrument then existing at the Observatory.

This instrument was fixed in its place by Messrs. TROUGHTON and SIMMS, in July 1833; and although when first erected it was not complete in some of its minute parts, yet, by various improvements which have been made, it is now approaching to a perfect state.

During the course of the last summer I made a great many observations with it, with the view of determining the most advantageous method of using it. To describe this method, and not the instrument itself, is the object of the present brief communication.

Among various experiments that occurred to me, I was led to a mode of observing which has not, that I am aware of, been suggested or put in practice by any other observer; and which appears to me to possess advantages sufficient to justify my making it the subject of a separate communication.

These relate, not only to the determination of the zenith distance of γ Draconis, (for which purpose the instrument was especially constructed,) but to the measurement of the minute variations from which the equations of aberration, parallax, nutation, and others employed in the reduction of the star, are to be elicited.

I shall first treat the subject with reference to the zenith distance of γ Draconis, presuming that the usual mode of using a zenith sector, altitude and azimuth instrument, or other instrument constructed on the same principle, is well known.

If the star be observed on one night with the instrument facing the east, and on the next or any subsequent night with the instrument turned half round, and facing the west, the double zenith distance will be obtained, on the supposition that the instrument has continued identical during the interval.

If, however, either by accident or design, the instrument should have suffered any change between the two observations, it is evident that the result will not give the measure of the required distance. I am now to show how my mode of observation is adapted to overcome this difficulty.

It so happens, that a small star of about the fifth magnitude, having nearly the same zenith distance towards the south that γ Draconis has towards the north, passes the meridian between 20 and 30 minutes in time after it. It is the different modes of employing this star in combination with γ Draconis, as a means of determining the various smaller equations, which I now wish to explain.

The angular distance between the two stars will be determined with this instrument in the usual manner, by observing them on the same night, and in the same position of the instrument; which distance in this case is the sum of the zenith distances of the two stars: but if, on the next or some following night, γ Draconis be observed, and after its passage the instrument be turned half round, and the other star observed, then the difference of the measure, as read on the micrometer, will be the difference of the zenith distances of the two stars. Thus, the sum being ascertained on one night, and the difference on another, these sums and differences will be independent of any change that may happen to the instrument from one night's observation to another; and the zenith distance of each star respectively may be deduced from these data. Whatever may be the superiority of this method of observation in ascertaining the zenith distance of the principal star, it is inconsiderable compared with the powerful assistance it affords in determining, with almost unlimited precision, the value of the small equations which necessarily become the subject of investigation.

Let it be supposed that the two stars have precisely the same zenith distance, the one to the north and the other to the south, then it is evident that if after the observation of one the instrument be turned half round, the micrometer wire will be placed in the exact position for bisecting the second star in its passage; but if the two stars have not exactly the same zenith distance, the micrometer wire will require a corresponding alteration. The distance between the two positions of the wire I call the subsidiary angle. It is to the properties of this angle that I wish to direct attention.

Whoever considers the nature of this angle, will perceive that it is measured by a very small motion of a micrometer screw, and therefore may be obtained with great precision: moreover, that any equation which may become the subject of consideration will be doubled in its effect on this subsidiary angle, and quadrupled when each star is affected equally by the same equation*.

This property of the subsidiary angle may be illustrated by observing, that the new instrument stands about seventy feet north of the principal meridian instruments of the Observatory. This produces in the zenith distance of each star a corresponding variation of about three quarters of a second; but the subsidiary angle will be altered by double that quantity, or a second and a half. It is probable that this

* Suppose the subsidiary angle equal $1' 00''$ when the aberration is nothing, and that the maximum of this equation is equal to $20''{\cdot}5$, then the extremes of the subsidiary angle will be $0' 19''$ and $1' 41''$, the difference of which is $1' 22''$, or quadruple of $20''{\cdot}5$, the equation to be investigated.

property may at some future period be applied with advantage in investigations made with moveable zenith instruments.

For the present purpose it is only essential to remark, that for the investigation of these small equations it is by no means required to have determined either the exact zenith distance of either star, or the exact difference of their zenith distances, or the absolute magnitude of this subsidiary angle, its variation from time to time being the only important object of research.

It is the more necessary to keep this in mind, as occasions may arise when it may be found advisable to omit altogether the investigation of the real zenith distance, and to confine the attention to the variations only of this small angle.

The improving performance of the zenith telescope leads me to hope that ere long I shall be able to illustrate the principle of the method of observation I have described, by a series of observations made with it; and should I not be disappointed in my expectations, the instrument will, I am of opinion, be found to rank among the most important in the Observatory.

The principles above explained apply with equal correctness to altitude and azimuth instruments, and may be advantageously adopted in their use.

Example.

Date.	Observed Subsidiary Angle.	Equations.	Reduced Subsidiary Angle.
1833.	′ ″	″	′ ″
July 26.	1 26·61	26·19	1 0·42
August 1.	1 30·99	29·13	1 0·86
3.	1 30·85	30·05	1 0·80
4.	1 31·26	30·50	1 0·75
11.	1 34·46	33·46	1 1·00
13.	1 34·94	34·24	1 0·70
14.	1 35·60	34·61	1 0·99
		Mean	= 1 0·79
The angular distance between the two stars, or sum of their zenith distances			= 3 3·53
		Sum.....	= 4 4·32
Half the sum, or zenith distance of γ Draconis			= 2 2·16
		Difference	= 2 2·74
Half the difference, or zenith distance of the small star			= 1 1·37

The angular distance between the two stars, or sum of their zenith distances, was obtained as follows:

			′	″
1833.	August	23.	3	3·11
		25.	3	3·89
		26.	3	3·87
	September	4.	3	3·41
		5.	3	3·60
		6.	3	3·68
		7.	3	3·65
		Mean.....	3	3·53

This last angular distance remains nearly constant throughout the year, except in the case of a parallax supposed greater in the one star than in the other, the rest of the equations being nearly the same for each star.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

MDCCCXXXIV.

METEOROLOGICAL JOURNAL FOR JULY, 1833.

1833. July.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☽ 1	29.869	75.5	29.835	69.9	47	62.2	61.6	53.7	66.6	0.050	SW	A.M. Fine and clear. P.M. Heavy thunder showers, with hail and brisk wind.
○ ♂ 2	29.953	72.4	29.968	68.7	47	61.2	65.0	47.3	67.0	0.272	W	Fine and clear—light clouds.—Evening, very calm.
☿ 3	30.041	69.8	30.071	68.2	47	63.2	64.3	50.2	66.7		W	Fair—cloudy and overcast.—Evening, fine.
♃ 4	30.204	73.6	30.174	70.2	48	65.8	72.7	51.7	73.7	0.094	SSW	Fine.—A.M. Cloudless. P.M. Cloudy.
♀ 5	30.171	67.3	30.095	70.4	56	63.9	71.0	56.6	73.3	0.011	SSW	Fine—lightly cloudy.
♄ 6	29.867	68.7	29.775	70.4	55	68.9	71.5	55.3	73.8		E	Fine.—A.M. Light high clouds. P.M. Cloudless.
☉ 7	29.657	68.7	29.697	70.9	56	67.5	67.8	61.3	69.7		E	Lightly overcast.—Light rain, P.M.
☽ 8	29.827	66.2	29.893	68.7	57	61.6	66.2	58.3	68.6	0.105	W	Overcast—showery.—Light breeze at night.
♂ 9	30.116	65.2	30.120	68.4	52	60.2	63.7	54.8	65.8	0.400	N	Cloudy—light unsteady airs.—Evening, fine and clear.
☿ 10	30.053	70.7	29.991	70.3	55	65.0	73.2	52.7	73.4		NNW	Fine—lightly cloudy.
♃ 11	29.950	68.6	29.922	71.3	60	64.5	68.0	57.6	71.9		NNW	Lightly cloudy and overcast.—Light shower, P.M.
♀ 12	29.867	65.3	29.838	67.0	53	58.5	63.6	55.6	64.5		E	Overcast—light wind.
♄ 13	29.933	62.4	29.970	66.0	51	56.7	61.1	52.9	61.7		N	Overcast—light wind.
☉ 14	30.012	65.6	29.968	66.7	53	59.8	65.2	55.2	70.4		N	Overcast.
☽ 15	30.031	73.0	30.073	68.9	56	63.0	69.7	58.3	71.4		SE	A.M. Cloudy. P.M. Fine—light clouds and wind.
♂ 16	30.232	72.3	30.223	70.7	54	65.2	73.6	56.0	74.2		N	Fine and cloudless.
● ☿ 17	30.241	71.6	30.220	73.2	62	69.0	78.0	58.6	80.4		SW	A.M. Fine. P.M. Cloudy.
♃ 18	30.184	71.0	30.109	74.5	61	70.5	78.4	63.7	80.4		S	Lightly cloudy.—Fine, P.M.
♀ 19	29.905	71.1	29.802	73.2	58	66.5	70.6	62.8	74.8		SW	A.M. Fine. P.M. Heavy rain at 2¼ h.
♄ 20	29.665	73.3	29.660	73.6	58	67.2	68.0	60.0	71.4		SW	Cloudy.—Light wind and rain, P.M.
☉ 21	29.778	70.6	29.821	70.7	53	62.1	64.4	53.3	66.3	0.092	W	A.M. Cloudy. P.M. Fine and clear.
☽ 22	29.827	65.7	29.693	68.6	58	59.0	67.8	52.2	69.7	0.161	SSW	A.M. Rain. P.M. Cloudy.—Evening, fine.
♂ 23	29.769	68.6	29.682	69.8	57	63.9	62.1	57.3	70.7	0.200	WSW	Overcast.—Heavy shower, P.M.—Evening clear.
☿ 24	30.065	70.7	30.108	69.6	50	63.3	67.2	51.4	67.8	0.297	NNW	Fine and clear—light clouds.
♃ 25	30.307	67.8	30.307	69.8	57	63.1	70.3	53.1	71.8		SW	Overcast.—Evening, fine and clear.
♀ 26	30.339	71.3	30.279	70.7	50	65.2	73.9	53.3	76.3		WSW	Fine and nearly cloudless.
♄ 27	30.301	72.4	30.283	73.9	63	71.4	79.6	63·2	80.5		W	Fine.—A.M. Light high clouds. P.M. Nearly cloudless.
☉ 28	30.326	73.9	30.301	75.6	64	71.7	75.3	64.7	78.4		NNE	A.M. Overcast. P.M. Fine and clear.
☽ 29	30.347	73.6	30.334	75.1	59	67.3	74.3	58.7	75.7		E	A.M. Light soft clouds. P.M. Clear and cloudless—light breeze.
♂ 30	30.461	70.4	30.455	73.0	58	61.0	67.2	56.3	70.7		N	Overcast—light wind.—Evening, clear.
○ ☿ 31	30.434	66.2	30.366	70.6	54	59.6	68.6	53.3	68.8		NNE	Fine—lightly cloudy.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.056	69.8	30.033	70.6	55.0	64.1	69.2	56.1	71.5	1.682		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.942 } { 3 P.M. 29.916 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

———————— above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR AUGUST, 1833.

1833. August.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☿ 1	30.392	65.7	30.404	70.2	50	61.4	65.3	57.3	66.4		NNE	Cloudy—light wind.
♀ 2	30.378	63.3	30.352	67.2	54	57.6	63.3	53.5	64.5		NNE	Overcast—light wind.—Light rain, early A.M.
♄ 3	30.301	63.3	30.314	67.4	54	60.0	64.2	54.8	64.7		N	Cloudy—light wind.
☉ 4	30.366	66.3	30.297	66.9	43	59.4	65.1	48.7	68.3		N	Fine and clear—light clouds.
☽ 5	30.111	64.4	30.087	68.0	51	63.0	67.4	51.7	68.2		NNW	A.M. Lightly cloudy. P.M. Fine and clear.—Shower at night.
♂ 6	30.172	66.3	30.150	67.4	47	58.8	65.3	49.4	66.7		NNE	Fine and clear—light clouds.
☿ 7	30.141	64.5	30.084	66.8	45	58.4	66.6	48.3	68.7		N	Fine.—A.M. Nearly cloudless. P.M. Cloudy—light wind.
♃ 8	30.091	66.3	30.071	67.6	50	62.2	67.2	52.8	69.7		W	A.M. Fine and cloudless. P.M. Cloudy. Evening, lightly cloudy—calm.
♀ 9	30.063	67.3	30.012	68.8	53	65.8	69.5	58.6	73.7		SSW	A.M. Fine—nearly cloudless. P.M. Lightly overcast.
♄ 10	30.031	67.3	29.978	69.7	55	63.8	70.5	55.2	73.6		W	Fine—light broken clouds.—Heavy rain at 6 P.M.
☉ 11	30.002	67.3	30.030	69.8	58	63.7	67.7	51.2	70.0	0.078	WSW	Fine—lightly cloudy.
☽ 12	30.210	66.2	30.154	68.7	49	59.7	67.0	49.8	68.3		W	Fine—light clouds—calm.
♂ 13	29.827	62.8	29.728	66.8	51	57.1	64.2	52.7	65.5		WNW	Overcast.
☿ 14	29.649	63·4	29.675	66.6	51	59.0	63.8	50.7	65.3	0.008	NNE	Fair—lightly cloudy.
● ♃ 15	29.768	62.2	29.760	65.8	52	59.3	65.2	50.2	66.3		N	A.M. Fine. P.M. Lightly overcast.
♀ 16	29.794	64.4	29.838	66.5	46	61.2	65.6	52.2	65.3		SSW	A.M. Cloudy. P.M. Fine.
♄ 17	29.918	64.3	29.848	66.4	49	60.2	67.3	50.6	69.9		SSW	A.M. Fine. P.M. Cloudy.
☉ 18	29.827	63.5	29.836	66.4	51	59.3	65.2	52.6	69.6		SW	A.M. Fine. P.M. Rain.
☽ 19	29.833	65.4	29.875	67.7	56	64.0	68.3	57.7	69.4	0.204	WSW	Fine—light clouds.
♂ 20	29.986	66.4	29.934	68.9	50	62.7	70.2	54.5	71.7		SW	Fine—lightly cloudy.
☿ 21	29.899	66.3	29.794	69.9	55	64.5	72.6	61.8	75.5		SW	A.M. Lightly overcast. P.M. Fine—light clouds.
♃ 22	29.782	67.8	29.722	69.1	47	60.3	67.8	52.0	68.6		SW	Fine—nearly cloudless.
♀ 23	29.658	66.6	29.707	68.8	48	61.1	67.8	51.0	68.3		SW var.	Fine—light clouds and wind.
♄ 24	29.964	64.2	29.990	68.2	53	61.3	68.7	54.2	69.4		W	Fine—lightly cloudy.
☉ 25	30.196	60.4	30.196	67.3	52	59.9	61.8	53.3	64.5		NE var.	A.M. Fine. P.M. Cloudy.
☽ 26	30.303	61.8	30.259	66.2	51	57.5	67.3	54.0	68.2		NW	A.M. Lightly overcast. P.M. Fine.
♂ 27	30.255	64.2	30.194	66.7	51	58.3	69.5	50.4	71.5		SW	Fine and cloudless—light wind.
☿ 28	30.257	65.7	30.225	68.9	54	63.2	72.8	53.0	73.7		SSW	Fine—light clouds.
♃ 29	30.159	67.4	30.010	69.6	59	61.5	72.8	55.3	75.3		S	Fine.—A.M. Light clouds. P.M. Cloudless.
○ ♀ 30	29.893	66.7	29.713	69.0	52	61.3	66.2	56.4	68.2		SW	A.M. Cloudless. P.M. From 6½ h. rain and high wind.
♄ 31	28.970	61.2	29.011	60.3	51	52.5	50.9	48.2	52.5	0.277	SW var.	Light rain, with high wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.004	64.9	29.976	67.7	51.2	60.6	66.7	53.0	68.4	0.567		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.904 | 3 P.M. 29.867 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

_____ above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR SEPTEMBER, 1833.

1833. Septr.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☉ 1	29.467	57.5	29.598	59.8	41	51.3	55·0	48.0	55.3	0.113	ESE	Fine—lightly cloudy.
☽ 2	29.958	55.8	29.950	59.7	39	50.2	60.2	41.0	61.9		SW	Fine.—A.M. Cloudless. P.M. Light clouds.
♂ 3	29.707	58.5	29.610	59.8	53	56.6	57.9	49.8	61.2		SSW	Fine—light clouds.—Heavy rain at 1½ P.M.
☿ 4	29.944	57.2	30.056	59.3	45	54.0	57.4	47.2	58.6	0.113	NE var.	Fine—light clouds and wind.
♃ 5	30.305	54.7	30.299	58.4	45	52.3	59.7	43.0	59.3		NNE	Fine—light wind.—A.M. Nearly cloudless. P.M. Light clouds.
♀ 6	30.281	56.2	30.181	59.9	49	54.9	62.8	45.4	63.3		NE var.	Fine—light wind.—A.M. Light clouds. P.M. Cloudless.
♄ 7	30.059	57.0	29.996	62.5	53	57.4	66.0	49.6	67.2		NE var.	Fine—lightly cloudy.
☉ 8	29.816	58.0	29.804	61.9	52	55.2	61.8	52.9	62.8		SE var.	Overcast—light wind.—Light rain, A.M.
☽ 9	29.899	59.8	30.027	63.2	55	57.9	64.3	54.8	64.8	0.025	NNW	A.M. Lightly overcast. P.M. Fine—light clouds.
♂ 10	30.083	60.7	30.027	61.5	53	57.6	59.8	56.4	60.2		ENE	Lightly cloudy and overcast.
☿ 11	29.792	60.7	29.703	64.5	56	58.7	65.2	55.8	66.8		NNW	A.M. Overcast—very light rain. P.M. Fine—light clouds.
♃ 12	29.973	59.8	30.033	62.0	45	55.5	60.3	48.2	61.2		SSW	A.M. Cloudy. P.M. Fine.
● ♀ 13	30.146	57.2	30.097	61.6	43	52.9	61.0	43.0	62.4		SW	Fine.—A.M. Nearly cloudless. P.M. Light clouds.
♄ 14	29.922	59.6	29.901	63.5	51	59.4	65.7	52.2	66.3		NE var.	A.M. Fine. P.M. Cloudy.
☉ 15	29.942	58.8	29.903	62.5	50	53.0	63.8	46.0	64.7		SSW	Fine—nearly cloudless.
☽ 16	29.689	61.0	29.641	63.4	52	58.2	62.0	52.4	64.4		SW	A.M. Fine. P.M. Cloudy.—At night, rain.
♂ 17	29.530	59.7	29.623	63.3	53	56.4	62.6	52.8	63.5	0.185	ENE	Cloudy.—Light rain, early A.M.
☿ 18	29.778	58.7	29.812	62.3	52	54.3	59.6	47.5	62.2		SW	Fine—light clouds.
♃ 19	30.043	57.7	30.081	61.4	48	53.8	59.2	45.5	61.7		SW	A.M. Fine. P.M. Light rain.
♀ 20	30.253	56.0	30.235	60.0	50	50.6	60.6	45.0	61.2	0.041	ENE	A.M. Fine. P.M. Lightly cloudy.
♄ 21	30.188	57.3	30.107	60.7	50	52.8	61.6	49.9	63.2		E	A.M. Light fog. P.M. Fine.
☽ 22	30.053	57.6	29.990	61.5	49	53.2	62.6	50.0	63.7		E	A.M. Fog. P.M. Fine—light clouds.
☽ 23	29.877	58.8	29.808	63.6	53	56.2	63.2	51.4	64.6		ESE	Fine—light haze and clouds.
♂ 24	29.443	60.2	29.415	62.0	55	59.6	61.2	53.2	61.5		NW var.	Overcast—light rain and brisk wind.—At night, high wind.
☿ 25	29.707	61.7	29.754	63·9	52	59.9	62.4	55.0	64.5		S	Fine—nearly cloudless—brisk wind.
♃ 26	29.798	61.2	29.839	64.2	54	59.0	61.5	54.2	63.7		S	Fine—nearly cloudless.
♀ 27	29.895	57.7	29.819	61.6	46	49.9	61.0	46.0	61.5		SSW	A.M. Fog. P.M. Fine.
○ ♄ 28	29.609	59.7	29.581	61.7	57	57.4	58.4	49.4	59.3		ESE	A.M. Cloudy. P.M. Fair.
⊙ 29	29.934	57.7	30.010	61.3	53	52.6	60.6	46.3	61.5		WSW	A.M. Fine and cloudless. P.M. Lightly cloudy.
☽ 30	30.254	57.3	30.260	60.7	50	50.7	61.7	48.8	62.3		ESE	A.M. Fog. P.M. Fine—light clouds.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.912	58.5	29.905	61,7	50.1	55.1	61.3	49.4	62.5	0.477		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr..... { 9 A.M. 29.830 | 3 P.M. 29.813 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

_____ above the mean level of the Sea (presumed about)..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR OCTOBER, 1833.

1833. October.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♂ 1	30.303	55.7	30.264	57.7	45	47.7	55.8	45.3	56.7		E	A.M. Fog. P.M. Fine—light clouds and haze.
☿ 2	30.239	55.3	30.154	58.4	47	48.2	60.3	46.8	60.3		E	A.M. Light fog. P.M. Fine—light clouds and wind.
♃ 3	30.063	56.2	30.039	57.4	51	51.8	55.2	47.7	55.1		NNE	Haze.
♀ 4	30.142	55.2	30.106	58.8	51	51.3	59.6	45.6	60.4		NNE	Fine.—A.M. Cloudless. P.M. Light clouds.
♄ 5	30.119	54.7	30.122	58.6	52	52.2	58.4	45.5	58.8		NNE	Lightly cloudy and overcast.
☉ 6	30.149	55.7	30.196	58.7	53	53.4	60.7	49.3	61.3		NNE	A.M. Overcast. P.M. Fine—light clouds.
☽ 7	30.158	55.2	30.095	58.4	51	50.9	59.7	48.3	59.7		NNE	A.M. Overcast. P.M. Fine and clear —light clouds and wind.
♂ 8	30.032	55.4	29.990	58.7	53	52.7	60.7	47.6	60.8		NNE	A.M. Light fog. P.M. Fine—light clouds.
☿ 9	30.166	55.3	30.202	58.3	51	51.0	57.4	48.3	57.4		NNE	Lightly cloudy and overcast—light wind.
♃ 10	30.243	54.9	30.170	57.4	49	53.5	55.7	47.5	56.5		E	Fine and cloudless.
♀ 11	29.994	52.7	29.948	56.4	43	47.2	57.2	43.7	58.3		E	A.M. Fog. P.M. Fair—lightly overcast.
♄ 12	29.950	54.3	29.871	57.8	50	50.6	58.2	46.7	58.8		WSW	Lightly overcast.—Evening, light rain.
● ☉ 13	30.109	51.0	30.039	55.3	41	43.8	51.9	38.6	57.6		WSW	A.M. Fine and cloudless. P.M. Fair —lightly cloudy.
☽ 14	29.540	55.8	29.478	58.6	57	57.7	57.7	43.3	59.7	0.014	SSW	Rain.
♂ 15	29.225	55.3	29.128	57.6	44	49.7	53.7	47.8	54.3	0.014	SW	Fine and clear—light clouds.—Evening, light rain.
☿ 16	29.215	51.8	29.294	53.8	45	45.4	50.3	41.3	50.3	0.017	W	Lightly cloudy.—At noon, fine and clear: evening, light rain.
♃ 17	29.556	51.4	29.633	53.7	49	49.1	51.7	43.2	52.6	0.028	NW	A.M. Light rain. P.M. Overcast.
♀ 18	29.668	50.3	29.547	53.3	43	45.7	51.2	41.2	51.2	0.008	W	Lightly cloudy and hazy.—Light wind, A.M.
♄ 19	29.433	50.9	29.893	54.2	47	47.8	51.3	42.8	53.3	0.031	W	Fine—lightly cloudy.
☉ 20	29.572	48.6	29.715	52.3	41	41.7	50.8	38.3	55.5	0.008	W	Fine—haze.—A.M. Cloudless. P.M. Light clouds and wind.
☽ 21	29.671	52.3	29.627	55.4	55	55.8	57.7	41.2	57.7		S var.	Overcast.—At night, rain with strong wind.
♂ 22	29.677	57.3	29.692	60.3	56	56.0	59.3	54.8	60.7	0.028	SSW	Fair—lightly overcast.—At night, rain with strong brisk wind.
☿ 23	29.430	57.6	29.507	59.9	55	56.9	56.6	53.2	59.3	0.014	S	Light wind and broken clouds.
♃ 24	29.686	55.7	29.576	59.6	52	53.3	61.8	47.3	61.7	0.008	ESE	A.M. Lightly cloudy. P.M. Light clouds and wind.
♀ 25	29.470	58.3	29.428	60.6	53	55.9	60.5	53.3	61.7		E	Fine—light high clouds.—Evening, cloudless.
♄ 26	29.567	58.9	29.522	61.2	54	56.0	60.9	54.7	62.3		ESE	A.M. Fine and clear. P.M. Overcast. Evening, light rain.
☉ 27	29.875	57.7	29.847	60.7	51	52.1	59.3	48.8	59.4	0.006	ESE	A.M. Haze. P.M. Fine—light ficeculous clouds—light unsteady wind.
○ ☽ 28	29.776	59.2	29.744	61.6	56	56.5	60.5	51.8	60.5		E	A.M. Light fog and deposition. P.M. Fine.
♂ 29	29.729	58.8	29.878	61.9	55	55.8	62.3	52.7	62.3		E	Fine—nearly cloudless.—Haze, A.M.
☿ 30	30.011	58.3	30.008	60.4	52	52.6	60.7	49.7	61.3		E	A.M. Haze. P.M. Fine and cloudless.
♄ 31	30.071	56.4	29.999	58.4	48	48.2	54.5	47.2	56.7		W	Fog.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.834	55.0	29.827	57.9	50.0	51.3	57.1	46.9	58.1	0.176		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.762 | 3 P.M. 29.746 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

———— above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR NOVEMBER, 1833.

1833. Nov.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♀ 1	29.840	58.3	29.841	60.3	57	57.2	58.2	47.7	59.7	0.007	WSW	A.M. Rain, early. P.M. Fine and cloudless.
♄ 2	30.056	55.4	29.804	58.3	47	49.0	55.2	45.3	56.3	0.008	WSW	A.M. Lightly cloudy. P.M. Fine.
☉ 3	29.721	54.7	29.858	55.7	41	48.8	51.6	45.8	51.6		WNW	A.M. Fine and cloudless—light brisk wind. P.M. Cloudy.
☽ 4	30.135	49.8	30.144	52.4	38	40.8	48.2	37.3	49.2		WSW	Fine.—A.M. Nearly cloudless. P.M. Light clouds.
♂ 5	30.284	48.7	30.134	51.0	41	44.5	50.3	37.3	53.7		WSW	Overcast.—Light rain, P.M.
☿ 6	30.004	53.7	29.990	57.6	52	54.6	55.8	43.7	57.4		WSW	Overcast and cloudy—light wind.
♃ 7	29.853	51.9	29.501	51.7	43	46.8	43.6	43.6	46.8		WSW	Rain.—Dark and lowering, P.M.
♀ 8	29.695	46.6	29.819	48.4	36	38.7	44.7	35.5	44.7	0.556	NNW	Fine—light clouds.
♄ 9	29.998	45.9	30.053	47.4	38	39.7	42.9	37.2	45.8	0.008	WSW	A.M. Haze. P.M. Fine—light clouds.
☉ 10	30.122	47.3	30.110	49.7	43	46.9	51.1	37.8	51.1		SSW	Lightly cloudy and overcast—light airs.
● ☽ 11	30.076	50.3	30.053	51.5	45	48.6	50.2	46.4	50.7		S	Overcast.—Light rain, P.M.
♂ 12	30.223	49.7	30.224	51.2	42	40.7	45.9	41.3	45.9		SW	Strong fog.
☿ 13	30.303	49.4	30.263	50.4	45	45.1	47.0	40.3	47.0		SW	Lightly overcast and hazy.
♃ 14	30.158	45.3	30.122	46.6	38	38.2	42.1	35.2	42.1		NE	Fine—light haze.—Cloudless, A.M.
♀ 15	30.071	44.4	29.976	44.9	35	37.8	40.3	36.3	40.3		E	Overcast—light fog.
♄ 16	29.874	44.8	29.867	47.2	38	41.3	45.0	37.4	45.7		S	Lightly cloudy—haze.—Light rain at night.
☉ 17	29.986	46.7	30.036	50.0	44	47.2	51.9	40.7	52.4		SSW	A.M. Cloudless—fog—deposition. P.M. Lightly cloudy.
☽ 18	30.291	50.6	30.293	52.0	48	50.3	52.7	46.4	52.7		ESE	Overcast.—Light fog and deposition, A.M.
♂ 19	30.251	52.7	30.198	53.8	49	49.7	50.2	49.2	50.3		W	Overcast—light fog.
☿ 20	30.109	49.1	30.057	51.3	40	40.8	46.3	40.3	48.7		SW	Light fog.—A.M. Overcast. P.M. Cloudless.
♃ 21	30.057	51.2	29.964	52.5	45	45.6	49.7	40.7	53.7		W	Lightly cloudy.—Light fog, A.M.
♀ 22	29.647	53.7	29.469	55.2	52	53.7	54.8	45.0	54.8		SSW	Cloudy—light wind.
♄ 23	29.723	51.8	29.724	53.3	44	45.3	48.4	43.7	48.4		WSW	A.M. Lightly overcast. P.M. Fine.
☉ 24	29.604	49.8	29.519	51.2	45	45.8	48.1	40.5	48.4		SSW	A.M. Fine and clear—light cloudiness. P.M. Dark—showery.
☽ 25	29.691	46.7	29.788	47.5	36	38.1	42.6	36.5	42.6	0.122	WSW	A.M. Cloudless—light fog and deposition. P.M. Lightly cloudy.
♂ 26	30.155	43.3	30.134	44.9	31	34.1	40.3	31.6	41.4	0.067	SSW	A.M. Fog and hoar frost. P.M. Fine—nearly cloudless.
○ ☿ 27	29.889	45.4	29.738	46.0	39	41.4	40.3	33.2	42.7		ESE	Fine—light wind.—A.M. Lightly cloudy. P.M. Cloudless.
♃ 28	29.405	44.4	29.058	46.7	42	42.7	47.7	34.6	50.6		SSE	A.M. Overcast—light fog and deposition. P.M. Rain.
♀ 29	29.227	47.3	29.441	48.3	37	44.4	47.2	42.3	47.2	0.031	WSW	Light wind.—A.M. Fine—lightly cloudy. P.M. Lt. rain. Evening clear.
♄ 30	30.029	45.3	30.013	47.5	39	41.1	46.3	38.7	51.3	0.009	W	Fine—light haze.—Cloudless, A.M.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.949	49.1	29.906	50.8	42.3	44.6	48.0	40.4	49.1	0.808		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.896 | 3 P.M. 29.847 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

——————————— above the mean level of the Sea (presumed about) ..... = 95 feet.

The external Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR DECEMBER, 1833.

1833. Dec.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☉ 1	30.140	49.4	30.062	50.8	47	51.6	53.6	40.3	54.3		SW	A.M. Light fog and deposition. P.M. Overcast.—Evening, light rain.
☽ 2	30.122	51.3	30.085	51.8	47	48.6	48.0	46.7	54.3	0.006	WSW	A.M. Overcast. P.M. Fine.
♂ 3	29.971	49.7	29.942	51.7	44	46.3	50.8	41.7	52.3		WSW	A.M. Lightly cloudy—light wind. P.M. Overcast.
☿ 4	29.656	51.8	29.540	54.3	51	52.8	53.2	45.8	53.2		SSW	Cloudy—light wind.
♃ 5	29.511	53.4	29.475	55.3	48	48.5	48.2	47.4	48.5	0.019	SW	A.M. Lightly cloudy. P.M. Fine.—At night, rain and unsteady wind.
♀ 6	29.579	49.4	29.711	50.6	39	39.9	46.7	38.7	50.7	0.014	WSW	Fine.—A.M. Cloudless. P.M. Light clouds.
♄ 7	29.392	50.7	29.314	52.4	50	51.5	51.5	39.3	54.3	0.006	SSW	Rain, with light unsteady wind.
☉ 8	30.069	46.7	30.088	48.3	37	38.8	43.7	37.2	53.3	0.011	W	Fine—light haze.—A.M. Cloudless. P.M. Lightly cloudy.—Rain at night.
☽ 9	29.564	50.7	29.564	51.9	50	53.9	55.6	37.9	55.6	0.008	WSW	Light rain.
♂ 10	29.881	49.6	29.808	50.2	41	42.3	46.2	41.4	46.2	0.006	SW	Fine.—A.M. Cloudless. P.M. Light clouds.
● ☿ 11	29.685	47.3	29.673	47.9	36	40.6	43.8	38.9	43.8		NNW	Light haze.—A.M. Cloudless. P.M. Fair—lightly overcast.
♃ 12	29.775	43.8	29.848	45.3	37	37.8	40.4	34.7	40.4	0.006	W	Fine—nearly cloudless—light wind.
♀ 13	30.072	42.3	30.111	43.5	35	36.3	39.8	32.9	41.9		WNW	Light haze.—A.M. Lightly cloudy—deposition—hoar frost. P.M. Fine—nearly cloudless.
♄ 14	30.132	44.4	30.128	46.6	41	42.9	46.5	35.5	47.3		WSW	A.M. Overcast—fog. P.M. Fine—light clouds.
☉ 15	30.038	47.4	29.983	52.3	43	43.4	48.8	42.4	52.3		WSW	Overcast—light fog—deposition.
☽ 16	29.732	50.7	29.522	51.6	48	52.1	53.2	48.3	52.3		WSW	Overcast—light unsteady airs.—A.M. Deposition. P.M. Light rain.
♂ 17	29.389	50.3	29.562	50.8	43	46.4	47.7	42.9	52.8	0.008	WSW	Strong unsteady wind, early A.M.—Fine—lightly cloudy.
☿ 18	29.417	51.4	29.701	52.4	50	52.8	50.0	42.5	52.7	0.008	W	Lightly cloudy.—Rain at night.
♃ 19	29.789	53.3	29.643	54.5	51	53.3	53.6	46.8	54.5		SSW	Overcast.—Evening, gale of wind with rain.
♀ 20	29.386	52.8	29.435	52.4	41	47.8	47.6	46.7	48.3	0.008	WSW	Fine—light clouds.—Morning and night, unsteady wind.
♄ 21	29.342	47.8	29.576	49.0	40	40.2	43.6	36.7	43.6	0.383	WNW	A.M. Lightly cloudy. P.M. Fine—nearly cloudless.
☉ 22	29.594	46.7	29.369	48.5	41	42.4	48.3	37.3	52.4	0.008	SSE	Light rain.
☽ 23	29.342	49.5	29.311	49.6	45	46.4	46.8	41.7	48.5		E	Fog—dark and overcast—heavy continued rain.
♂ 24	29.404	50.0	29.311	51.7	49	49.3	51.0	43.8	51.7	0.944	S	Lightly cloudy and overcast.—Light wind and deposition, A.M.
☿ 25	29.457	50.0	29.748	49.4	44	46.7	43.4	43.8	46.7	0.083	NNW	A.M. Light fog.—Noon, rain. P.M. Clear—light clouds.—Evening, very clear.
○ ♃ 26	30.176	44.3	30.041	45.3	33	35.6	42.5	34.3	46.0		WSW	Lightly overcast—light fog.—Evening, rain.
♀ 27	29.796	46.8	29.744	48.1	43	43.8	46.8	34.6	46.8	0.133	WSW	A.M. Fog deposition. P.M. Fine—nearly cloudless.
♄ 28	29.952	44.8	29.988	46.4	35	37.3	42.9	35.5	51.4	0.061	WSW	Lightly cloudy and overcast.—Light fog and deposition, A.M.
☉ 29	29.746	49.6	29.798	51.0	46	51.8	51.3	36.5	52.3	0.011	W	Fine—cloudy.—Light wind, A.M.
☽ 30	29.665	50.9	29.659	53.2	50	51.8	54.5	47.8	54.7		WSW	Rain.
♂ 31	29.641	50.7	29.474	52.2	49	50.3	49.6	43.6	53.2		SSW	A.M Drizzling rain. P.M. Fine—nearly cloudless.
	Mean 29.722	Mean 49.0	Mean 29.710	Mean 50.3	Mean 43.7	Mean 45.9	Mean 48.1	Mean 40.8	Mean 50.3	Sum 1.723		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.661 | 3 P.M. 29.650 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

——————————— above the mean level of the Sea (presumed about)..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.